VOL. LVI. NO. 223.] [July, 1947

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—NEW FOUNDATIONS FOR LOGIC.

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1. Introduction.[1] Logic ought to be simple; and, in a way, even trivial. Complications in logic all arise from two sources. Reiterated applications of trivialities may result, in the end, in such complexity that the thread is lost, as it were, and that one has to resort to the laborious method of carefully checking every step in order to ensure that all is well. A minute analysis is thus often made necessary, and this creates the other source of complication—the need for a high degree of precision in the formulation even of trivialities.

It is assumed here that the central topic of logic is the *theory of formal or deductive inference* (or of derivability; or of deducibility; or of logical consequence: all these expressions are here taken to mean the same thing). We shall first attempt to determine this notion of logical derivation or derivability by laying down a few very simple primitive rules for it. This will be done in sections 2 and 3.

In spite of the triviality of these rules, the concept of derivability thus determined turns out to provide an exceedingly powerful instrument. It will be shown that with its help, we can draw up systems of rules of inference which cover not only

[1] This paper is a report and a discussion of some results obtained by the author during the last ten years. Formal proofs are avoided here but they will be published elsewhere. A knowledge of symbolic logic is not assumed; on the contrary, it may be necessary to warn readers against the attempt to identify our theory of compound statements and quantification with the calculi of propositions or functions. Our theory is more general: we do not construct one calculus or language, but we investigate a wide range of languages, among them some that do not contain any distinct symbols corresponding to those symbols which play such a role in the calculus of propositions and in similar calculi.

the theory of compound statements (corresponding to the "calculus of propositions") but also the theory of quantification, that is to say, of universal and existential statements (corresponding to the "lower functional calculus").

But we shall go even further. Surveying our system of rules of inference, we shall find that it is possible to lay down definitions of all the logical concepts, if we take derivability as our sole undefined specifically logical concept. But this means that all these other concepts,[1] in principle even though not in practice, can be dispensed with, and that the primitive rules of sections 2 and 3, in spite of their triviality, somehow cover the whole of the theory of inference.

2. General Theory of Derivation. If we wish to discuss rules which tell us under what circumstances a certain statement—the conclusion—follows from certain other statements—the premises—, then we have first to come to an agreement how to write such rules.

Take "*a*"; "*b*"; "*c*"; ... to be names of statements; then we can write

$$\frac{\begin{array}{l}\text{“}a\\ b\\ c\end{array}}{d\text{”}}$$

or something like it, in order to express the assertion: "From the premises, *a*, *b*, and *c* the conclusion *d* can be derived." This manner of writing a rule of derivation is known from traditional logic, where one often writes rules of inference (or "moods") in this way; for example, the *modus ponendo ponens* is often written:

$$\frac{\begin{array}{l}\text{“}\textit{If } A \textit{ then } B\\ A\end{array}}{B\text{”}}$$

which may be read: "From the premise '*If A then B*' together with the premise *A*, the conclusion *B* can be derived."[2]

[1] Except the concept "the result of substituting x for y in the formula a" which is introduced, in section 6, by way of a few exceedingly trivial primitive rules of inference (and which may be said to be implicitly definable in terms of derivability).

[2] For a precise rendering, Quine's signs for so-called "quasi quotation" should be used instead of our single quotation marks. But since we shall not make any use of this device in what follows, it seems preferable not to burden the reader with it.

In order to save space, and to make our way of writing less clumsy, we shall use here a slightly different notation ; the main difference being that we shall write everything in one line, using the symbol " / " instead of the horizontal line ; we shall, furthermore, use commas in order to divide the various premises, instead of writing them in different lines. In other words, the notation :

$$" a, b, c/d "$$

will be used in order to express the assertion : " From the statements a, b, and c, the statement d can be derived."

Very often we shall have to discuss inferences from many premises. Let n be the number of the premises we are considering. Then the expression :

$$" a_1, \ldots, a_n/b "$$

will be used for conveying the assertion : " From the statements a_1, a_2, a_3, . . . , a_n, the statement b can be derived."

The sign " / " designates the *specifically logical concept of deducibility* whose importance was emphasised in the *Introduction*. It should be noted that, although we may operate with as many premises as we like, we draw only *one* conclusion at a time ; in other words, an expression such as " $a_1, \ldots, a_n/b, c$ " has no meaning for us, as we are using our notation. If we wish to assert that b as well as c follows from $a_1, \ldots, a_n$, then we have to say : " $a_1, \ldots, a_n/b$ and $a_1, \ldots, a_n/c$ ".

Having explained our notation, we proceed to the discussion of actual rules of derivation. The most trivial and at the same time the simplest rule of inference is undoubtedly

$$a/a \tag{2.1}$$

in words : " From the statement a, the statement a can be derived."

This rule is so trivial that many classical logicians have hesitated to admit it. They have felt that nothing could be gained by admitting it, and that the dignity of the procedure of inference would be imperilled if this were called an inference. But these considerations are irrelevant. Since the rule is, obviously, not invalid, we should have to admit it even if it were useless. But, contrary to first impressions, it is far from being useless : within those more subtle analyses of more complicated threads to which we referred in the first section, it turns out to be exceedingly useful ; and it is even characteristic, altogether, of the triviality of the fundamental rules from which the edifice of logic arises. We should, furthermore, keep in mind that triviality of the basic

assumptions is an advantage rather than a disadvantage, provided that what we obtain, at the end, is adequate for our purpose.

Another trivial valid rule is the following:

(2.2) If $a_1, \ldots, a_n/b$ then $a_1, \ldots, a_n, a_{n+1}/b$

which can be translated into: "If an inference is valid, then adding a new premise does not make it invalid." Indeed, the addition of new premises can only strengthen the premises,—just as an omission, in general, weakens them. ("Strengthening" and "weakening" will be defined and discussed later in this section.)

A third triviality is that the *order* of the premises does not affect the validity of the inference. This leads us to lay down the rule

(2.3) If $a_1, \ldots, a_n/b$ then $a_n, \ldots, a_1/b$.

This rule permits us to reverse the order of the premises; by itself, it does not allow us to put the premises into any desired order. But, combined with the previous two rules, and with one (the principle of transitivity) which we shall soon discuss, it does, in fact, allow us to achieve precisely this. (This fact can be established; but we omit all such proofs in this paper.)

Before proceeding to the discussion of a more complicated rule, the principle of the transitivity of derivability, it should be mentioned that the three rules so far explained, *i.e.*, 2.1, 2.2, and 2.3, can (in the presence of the transitivity principle) be replaced by one single rule, of about equal simplicity, *viz.*, the following:

"From the premises $a_1, \ldots, a_n$, we can derive, as a conclusion, any statement a_i which is one of the premises, *i.e.*, for which $1 \leqslant i \leqslant n$." This can be expressed in our notation by

(2.4) $a_1, \ldots, a_n/a_i \quad (1 \leqslant i \leqslant n)$.

The notation might perhaps irritate some who are not used to this kind of thing; but it is only the notation which makes this rule appear less trivial than the others; the idea is simplicity itself, and the rule, obviously, is valid. For if (as we asserted) the order of the premises is irrelevant; if adding to them only strengthens them, and does not invalidate the inference; and if

$$a/a$$

is valid, then, obviously,

(2.41) $a_1, \ldots, a_n/a_1$

must be valid, and also

$$a_1, \ldots, a_n/a_2$$

provided n is, at least, equal to two ; and also

$$a_1, \ldots, a_n/a_3$$

provided n is at least equal to three ; and generally we have 2.4, *i.e.*,

$$a_1, \ldots, a_n/a_i$$

provided n is at least equal to i, which is precisely what we express by the condition "$1 \leqslant i \leqslant n$". We shall call 2.4 the "generalised principle of reflexivity" and refer to it by "(Rg)", *i.e.*, generalised reflexivity principle.

We now turn to the principle of transitivity. In its simplest form, this rule can be expressed by

(2.5) If a/b, then : if b/c then a/c

or in words : "If b follows from a, then : if c follows from b, then c follows from a." We shall refer to it by "(Ts)", *i.e.*, "simplest Transitivity principle".

This rule (Ts), although perhaps slightly less trivial, is still very simple. In the next section, we shall indicate a method by which it can be retained in the simple form in which we have introduced it here. This method, however, makes use of a new concept, that of the conjunction of two statements,—say, the statement a and the statement b. The introduction of this concept simplifies certain things very considerably—this is, indeed, the reason why it is introduced. But it goes hand in hand with a restriction of the generality of our approach. This is a serious step which should not be taken before we have seen how far we can get with our present method. Besides, only if we see that things get a little complicated can we appreciate the simplification achieved by the new method. We shall therefore proceed to explain the more complicated forms of the rule of transitivity which are needed in many inferences.

But we shall first introduce here some linguistic abbreviations. We shall write in our rules sometimes "$\rightarrow$" instead of "if . . . then" ; for example, we shall write a rule like 2.2 sometimes :

(2.2+) $$a_1, \ldots, a_n/b \rightarrow a_1, \ldots, a_n, a_{n+1}/b$$

or the rule 2.3 :

(2.3+) $$a_1, \ldots, a_n/b \rightarrow a_n, \ldots, a_1/b.$$

The introduction of the sign "$\rightarrow$" for "if . . . then" is a very different matter from the introduction of a new logical concept. It is *merely* an abbreviation ; and in order to emphasise this fact, we shall quite frequently revert to our ordinary way of writing.

It is important to keep this in mind, and not to think that we introduce a logical symbolism in an underhand manner. (A new logical sign must be introduced explicitly by a rule or by rules determining its use. The only specifically logical sign we have so far introduced is " / ", and we are still busy explaining the rules, such as 2.1, 2.2, 2.3, etc., which determine its use.)

If we use our new abbreviation, rule 2.5, that is to say (Ts), becomes:

(2.5+) $$a/b \rightarrow (b/c \rightarrow a/c).$$

The brackets correspond to the colon of the original formulation.

Apart from " $\rightarrow$ " we shall make use of " $\&$ ", in the usual way, as abbreviation for " and "; again, we shall indicate the informal character of this arrangement by reserving the right of continuing to write " and " whenever we like.

We now proceed to the more complicated forms of the transitivity principle.

The simple form discussed so far allows us only to handle cases of inference from *one* premise. It can be extended to the case of more than one premise by writing, instead of 2.5+:

(2.5e) $$a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c).$$

In words: " If the statement b follows from the premises $a_1, a_2, \ldots, a_n$, then, if c follows from b, c also follows from $a_1, a_2, \ldots, a_n$." We shall refer to this form of the principle by " (Te) ", *i.e.*, " extended transitivity principle ". It is a very important principle, and it will be used, in the next section, together with (Rg) and another rule, to form what we shall call our " basis II ", which makes use of conjunction. But at present, we do not wish to use conjunction, and we need, therefore, a stronger and more complicated principle which we shall call the " generalised principle of transitivity " and which we shall denote by " (Tg) ".

This generalised principle of transitivity is, indeed, not so simple as our other principles, and this is the reason why we shall attempt to do without it, that is to say, why we shall design a method for avoiding its acceptance as a primitive rule.

Why we need it here is due to the following reason:

We may be able to derive the statements b_1 as well as b_2 from the premises $a_1, \ldots, a_n$; *i.e.*, we may have

$$a_1, \ldots, a_n/b_1 \ \& \ a_1, \ldots, a_n/b_2.$$

At the same time, we may have

$$b_1, b_2/c.$$

Clearly, we have then also

$$a_1, \ldots, a_n/c.$$

Or, putting it in one line, the following rule will be generally valid:

$$(a_1, \ldots, a_n/b_1 \;\&\; a_1, \ldots, a_n/b_2) \rightarrow (b_1, b_2/c \rightarrow a_1, \ldots, a_n/c).$$

Now this is perhaps not much more complicated than (Te). But we cannot stop here. It is possible that c does not follow from b_1 and b_2 together, but that it follows from the m premises $b_1, \ldots, b_m$; or in other words, it may be that

$$b_1, \ldots, b_m/c$$

holds; and it is quite possible that *each* of these m premises of c, *i.e.*, *each* of the statements, $b_1, b_2, \ldots, b_m$, follows in turn as a conclusion from the premises $a_1, a_2, \ldots, a_n$. In this case,

$$a_1, \ldots, a_n/c$$

will hold also.

But how shall we write down a rule stating this more complicated kind of transitivity? There is no very simple way of writing it down.

One way of writing it is this:

$$(2.5g) \quad (a_1, \ldots, a_n/b_1 \;\&\; a_1, \ldots, a_n/b_2 \;\&\; \ldots \;\&\; a_1, \ldots, a_n/b_m) \rightarrow \rightarrow (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c).$$

Another way of writing it is this:

$$(2.5g^1) \quad \left\{ \begin{array}{ll} & a_1, \ldots, a_n/b_1 \\ \& & a_1, \ldots, a_n/b_2 \\ \cdot & \cdot \quad \cdot \quad \cdot \\ \cdot & \cdot \quad \cdot \quad \cdot \\ \cdot & \cdot \quad \cdot \quad \cdot \\ \& & a_1, \ldots, a_n/b_m \end{array} \right\} \rightarrow (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c).$$

Or we may introduce a new abbreviation for any expression like

"$a/b_1 \;\&\; a/b_2 \;\&\; a/b_3 \;\&\; \ldots \;\&\; a/b_m$"

(whatever the number of the premises may be) by using instead the brief expression:

"$\prod_{i=1}^{m} a/b_i$"

This will be found repulsive by many, and we shall not continue to use anything as complicated as this in the remainder of the

paper. But we may mention that our rule 2.5g is in this way much easier to write:

$$(2.5g\dagger) \quad \prod_{i=1}^{m} a_1, \ldots, a_n/b_i \rightarrow (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c).$$

Another way out would be to write:

If, for all values of i between 1 and m (*i.e.*, for $1 \leqslant i \leqslant m$), we have $a_1, \ldots, a_n/b_i$, then we also have:

$$(2.5g\ddagger) \qquad \text{if } b_1, \ldots, b_m/c \text{ then } a_1, \ldots, a_n/c.$$

But whatever way of writing we may adopt—there is no doubt that this general principle of transitivity is considerably less simple and convincing than, say, (Te), *i.e.*,

$$(2.5e) \qquad a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c).$$

Before we proceed, in the next section, to develop a method which allows us to dispense with anything more complicated than the rules (Te) or even (Ts), I wish to make clear that the system consisting of the two rules (Rg) and (Tg), *i.e.*, 2.4 and 2.5g, or alternatively, the system consisting of the four rules, 2.1; 2.2; 2.3; and (Tg), is sufficient as a basis for the construction of propositional and functional logic, as undertaken in sections 3 to 7. (The precise sense in which these sets form a "basis" will become clear in the sequel.)

The two systems are of *equal effect* in the sense that it can be shown that every inference which is valid in the one is also valid in the other. Furthermore, each of the two systems is independent in the sense that it can be shown of any one of the rules so far mentioned that, if it is omitted, the system in question is no longer of equal effect with the other system. (They can also easily be shown to be consistent.)

With the help of each of the two systems, a large number of what we may call "secondary rules of inference" can be shown to be valid. A rule is called "secondary" to some other rules which are "primary" relative to it if it does not add anything new to these primary rules; that is to say, if every inference which is asserted as valid by the secondary rule could be drawn merely by force of the primary rules alone, for example, by reiterated application of one of the primary rules, or by applying one after the other. ("Primitive" rules are rules of a set of rules such that all the other rules considered are secondary to the rules of this set; and an independent set of primitive rules is called a "basis".) Many of the secondary rules of our system

are, of course, trivial, but some are not. Among the more trivial ones is the following (which we already know):

(2.41) $\quad a_1, \ldots, a_n/a_1$

This is clearly a valid rule whenever 2.4 is valid.

(2.6) $\quad$ If $a_1, \ldots, a_n/b$ then $a_{n+1}, a_1, \ldots, a_n/b$.

This rule is very similar to rule 2.2, but it does not follow from 2.2 alone. (To say that a rule "follows" from certain other rules is merely an elliptical way of saying that it is secondary to them.) On the contrary, the system consisting of rules 2.1 to 2.3 and (Tg) remains independent if 2.3 is replaced by 2.6 which shows that 2.6 is not only independent of 2.2 but also of 2.2 together with 2.1 and (Tg).[1]

Another secondary rule is one which we mentioned when introducing the rule 2.3 (which permits the reversal of the order of the premises). It is the rule that the premises may be rearranged in any desired order. This simple rule, if formulated in our usual way, looks a little complicated—again a case where the complication is not due to the contents of a rule but to the need of expressing it precisely, and in a handy and standardised way. The rule is:

(2.7) $\quad a_1, \ldots, a_{n+m}/b \rightarrow a_n, \ldots, a_1, a_{n+1}, \ldots, a_{n+m}/b$
$(0 \leqslant n \leqslant n+m)$.

Rule 2.7, together with 2.5 and 2.4, may be taken for another basis of equal effect with the ones mentioned.

An obvious but very important secondary rule is the following:

(2.8). If in any valid inference, one or more of the premises are such that they follow from the others, then they can be omitted as redundant, *i.e.*, without invalidating the inference.

In view of

$$a/a,$$

this rule 2.8 allows us to omit from a set of premises any premise which occurs more than once.

It will have been noticed that we have rules of two kinds. Some, such as "a/a" or "$a_1, \ldots, a_n/a_n$" may be called *absolute*

[1] I owe to Professor Paul Bernays the suggestion of replacing 2.3 by 2.7. It can be shown, however, that our original set 2.1 to 2.3 and (Tg) is in the following sense weaker than the new system (with 2.7 instead of 2.3). If, in our original system, 2.1 is replaced by the stronger rule 2.41, then the system remains independent. In the new system, if 2.1 is so replaced, 2.2 becomes dependent. (We thus obtain a system of three primitive rules, *viz.*, 2.41; 2.6; 2.4.)

rules of derivation; others such as "$a/b \rightarrow a,\ c/b$" (a secondary rule to 2.2) may be called *conditional* rules of derivation. In a conditional rule, we may distinguish between the antecedent rule and the consequent rule. Thus in the conditional rule

$$\text{"}a,\ b/c \rightarrow b,\ a/c\text{"},$$

the rule "$a,\ b/c$" is the antecedent rule, while "$b,\ a/c$" is the consequent rule.

Some important and no longer trivial principles can be shown to hold of all such conditional rules. In order to formulate them, we first explain the terms "strengthening" and "weakening" as applied to premises or conclusions.

If a conclusion can be validly derived from some premise(s), we say that it is therefore logically *weaker* than the premise(s) or at most equal (in strength) to them, and that the premise(s) are logically *stronger* than the conclusion or at least equal to it. We shall, for brevity, say simply "weaker" and "stronger", but we have to keep in mind that these terms are here intended to be used in such a way as *not to exclude equal strength;* for the validity of "a/a" reminds us that a conclusion may, very obviously, be as strong as the premise from which it is drawn.

We further say that we "strengthen" (or "weaken") the premise(s) or the conclusion of a rule if we replace some or all of them by others in such a way that as a result they become logically stronger (or weaker, as the case may be); equal strength, again, is not excluded.

Now we can formulate some of the secondary rules which can be shown to hold in our system as follows:

(2.9). In a valid conditional rule,

(*a*) the premises of the antecedent rule may be weakened,

(*b*) the conclusion of the antecedent rule may be strengthened,

(*c*) the premises of the consequent rule may be strengthened,

(*d*) the conclusion of the consequent rule may be weakened,

without affecting the validity of the conditional rule.

It is possible to show that any change of any valid conditional rule of our system, effected in accordance with rule 2.9, leads to a new valid rule which, too, is within our system.

Another interesting principle is connected with *mutual deducibility* (*or logical equivalence*) of statements. If a and b are mutually deducible, we write

$$\text{"}a//b\text{"}.$$

" $//$ " is a new concept of our system, and can be easily defined on the basis of " $/$ ", by the following obvious Definition :

(D$//$) $\quad a//b$ if, and only if, $a/b \;\&\; b/a$.

This definition may be abbreviated by using " $\longleftrightarrow$ " for " if and only if " :

(D$// +$) $\quad a//b \longleftrightarrow (a/b \;\&\; b/a)$.

We shall use " $\longleftrightarrow$ " quite often, but, in the same way as " $\rightarrow$ " and " $\&$ ", as an abbreviation only.

Now it can be shown of our system that, whenever $a//b$, we can, without impairing its validity, substitute a for b, in some or all places of the occurrence of b, in any valid inference. If we call two mutually deducible statements " logically equivalent ", then we can call this very general rule the *substitutivity principle for logical equivalence.* And we can read " $a//b$ " not only " a is logically equivalent to b ", but also " a is substitutionally equal to b " ; and we can call mutual deducibility also " *substitutional equality* ". (The problem of the substitutivity of logical equivalence will be mentioned again in later sections.)

But is our system of rules valid ?

We can define validity of inference (following in the main Tarski [1]) in the following way : An inference is valid if every interpretation (*i.e.*, for our present purpose : every actual set of statements which can be used as an example of one of our rules) which makes the premises true also makes the conclusion true.

If we call an interpretation which makes all the premises true and the conclusion false a *counter-example*, then we can also say : An inference is valid if no counter-example exists.[2]

Now it can be very easily shown of all our rules that they are valid in this sense. Take, for example, the rule 2.41 :

$$a_1, \ldots, a_n/a_1.$$

[1] *Cp.* A. Tarski, " Ueber den Begriff der logischen Folgerung ", Actes du congrès internat. de philos. scientifique, fasc. VII, Paris, 1936. We shall not discuss here the concept of interpretation. (Tarski speaks of " models ".) But the fact, established in this paper, that formative signs can be defined in terms of a concept of deducibility which does not assume any formative signs in turn, opens a way to applying Tarski's concept without difficulty. I have in mind the difficulty, mentioned by Tarski, of distinguishing between formative (" logical ") and descriptive signs. This difficulty seems now to be removed. (*Cp.* my " Logic without Assumptions," forthcoming in *Proc. Arist. Soc.*, 1947.)

[2] See also my " Why are the Calculuses of Logic and Mathematics Applicable to Reality ? ", in *Logic and Reality*, Aristotelian Society Supplementary Volume XX, 1946, esp. pp. 47 ff.

Assume that a counter example exists. Then there must be a certain interpretation which makes all the premises true and, at the same time, the conclusion false; thus, in the light of this interpretation, the conclusion a_1 will be a false statement. But since a_1 occurs in the premises also, they cannot possibly be all true statements. Accordingly, no counter example can exist.

In a similar way, all the other rules can easily be shown to be valid, and this explains their triviality: the degree of their triviality is, as it were, in proportion to the ease with which their validity can be shown.

3. Another Approach. Conjunction. We now proceed to discuss the replacement of the complicated rule (Tg) 2.5g by the simpler rule (Te) or 2.5e. But in order to do this, we must somewhat restrict the generality of our approach.

Indeed, the rudiment of a theory of inference sketched in the foregoing section[1] is about as general as such a theory can possibly be. It can be applied to any language in which we can identify statements (or sentences, or propositions—there is no need for us here to enter into the problem of the possibility or necessity of distinguishing between these entities), that is to say, expressions of which we might reasonably say that they are true or that they are false. Nothing is presupposed of our *a*, *b*, *c* ... except that they are statements, and our theory shows, thereby, that there exists a rudimentary theory of inference for any language that contains statements, whatever their logical structure or lack of structure may be. (But this means, for any *human* language, that is to say, for any language which is not only expressive and evocative of response, as animal languages are, but also descriptive, *i.e.*, containing means for describing facts, or statements of facts, which may be true or false.)

We may mention here in passing that our symbols "*a*"; "*b*"; "*c*"; etc., are *variables*, similar to those used by mathematicians in algebra; and as the values of the mathematicians' variables are *numbers*, so the values of our variables are state-

[1] Professor Bernays has drawn my attention to the close relationship that exists between this theory and the system of five axioms for the theory of consequence developed by A. Tarski in his "Ueber einige fundamentale Begriffe der Metamathematik", *Comptes rendus des Séances de la Soc. des Sc. et des Lett. de Varsovie*, 1930, Cl. iii. Bernays designed a method of translating Tarski's *prima facie* very different and more abstract approach into a more elementary one, very similar to the approach presented in this section. (He has also drawn my attention to a somewhat similar system; see Paul Herz, *Mathem. Ann.* 89, 1923, and *Erkenntnis* 2, 1932.) With the help of this method it can be easily shown that our system is equivalent to the first four of Tarski's five axioms.

ments. The student of algebra studies a universe of discourse consisting of numbers (of some kind—say, natural numbers, or real numbers). We study a universe of discourse consisting of statements (of some kind—say, the statements of a certain language). The mathematician allows that certain *figures* (not numbers)—for example, the figures "*1*", "*2*", etc., may be substituted for his variables; that is to say, what may replace his variables is any *name* of a number. Similarly, we must assume that *names of statements* (not the statements themselves) may be substituted for our variables. This is what we mean when we say that the mathematicians' symbols "*a*"; "*b*"; etc., are variable names of numbers, *i.e.*, variables which designate unspecified numbers; in the same sense, our symbols "*a*"; "*b*"; etc., are variables which designate unspecified statements, and they may be described, in the sense indicated, as variable names of statements.

Now the universe of statements envisaged in the foregoing section may consist of the statements of any language, whatever its logical structure. But in the present section, a certain restriction is imposed on the admissible languages—they must contain what we shall call *conjunctions*.

We shall define conjunction in what follows. The intuitive meaning of our definition can be conveyed perhaps in this way: We say that a language L contains conjunctions (or that it contains the operation of conjunction) if we can join any two statements—the statement a, say, and the statement b—in such a way as to form a new statement c which is logically equivalent to a and b together. This is done, in English, by linking them together with the help of the word "and". But we need not suppose that any such word exists: the link may be effected in very different ways; moreover, the new statement need not even contain the old ones as recognisable separate parts (or "components"). We may therefore say:

A language L contains the operation of conjunction if, and only if, it contains, for any two statements, a and b, a third statement c which is logically equivalent to them (*i.e.*, a little more precisely, equivalent to the first two statements taken together).

Now the remark on logical equivalence definitely needs here some clarification. According to the way in which we have introduced "$\vdash\!\vdash$", we cannot write "$a, b \vdash\!\vdash c$"; rather, only *one* statement is allowed to occur on the left- as well as on the right-hand side of "$\vdash\!\vdash$" only. For we must remember that our conclusions always consist of *one* statement only; and in the case of "$\vdash\!\vdash$", conclusions (as well as premises) stand on both sides.

Nevertheless, the statement c may be said to be equivalent to the two statements a, b if we have:

$$c/a \quad . \quad . \quad . \quad . \quad . \quad (1)$$
$$c/b \quad . \quad . \quad . \quad . \quad . \quad (2)$$
$$a, b/c \quad . \quad . \quad . \quad . \quad . \quad (3)$$

We are thus led to a definition along the following lines:

(D 3.01). The statement c is a conjunction of the two statements a and b if, and only if, c/a, c/b, and $a, b/c$.

(D 3.02). A language L contains the operation of conjunction if, and only if, it contains, with every pair of statements, a, and b, a third statement c which is the conjunction of a and b.

Definitions D 3.01 and D 3.02 are as such quite in order, and adequate. But in view of our aim—the simplification of the transitivity principle—we shall *not* adopt D 3.01. Rather, we shall replace D 3.01 by a similar but slightly stronger definition.

The situation is this: D 3.01 makes, of course, very essential use of our specifically logical concept " / ". Its force will depend, therefore, upon the force of this concept " / "; but this means upon the primitive rules of derivation assumed to hold. A closer analysis shows that D 3.01 is perfectly adequate as long as we assume all those rules of inference which we are accustomed, intuitively, to use; which means, in effect, the systems of primitive rules discussed in section 2. But if we drop some of these rules, or if we replace 2.5g by the weaker form of the transitivity principle 2.5e, then our definition is no longer quite adequate. It turns out, in this case, that it does not entitle us to assert that *more than two* separate statements can be joined into a conjunction or, in other words, that there exists, for any three statements a, b, and c, an equivalent statement d. On the basis provided by section 2, this can be shown to hold, but not if 2.5g is replaced by 2.5e or 2.5. (It is in these somewhat subtle points that our trivial system of logic becomes complicated.)

But the main point of using conjunction is just that it should permit us to *link up any number of statements into one.* If it does this, then we can always replace the m conclusions of m different inferences from the same premises by one inference. But if we can do this, then (Te) or perhaps even (Ts) will be, clearly, sufficient. At the same time, many other simplifications would be possible. For example, we could use " // " in order to express equivalence between $a_1, \ldots, a_n$ and $b_1, \ldots, b_m$—namely by first replacing each of the two sets by the conjunction of all statements belonging to it.

We see here that it is a practical need—the need for simplifica-

tion, abbreviation, greater lucidity of certain formulations, etc.—which leads us to introduce conjunction; and we may safely assume that it was precisely such a need which led, in the actual development of most human languages to the invention of a linguistic device, such as the use of the word "and", which guarantees that the language contains conjunction in the sense defined above.

Since our definition is, as we have said, only adequate on the basis of the whole system of section 2, we shall have to modify it in order to achieve our purpose. But before proceeding to do so, we shall make some comments upon another aspect of our definitions D 3.01 and D 3.02. These comments will go into some detail, but our labours will be rewarded, for our findings concerning conjunction apply equally to the hypothetical, the disjunction, the negation, the quantifiers, etc.

If we designate the conjunction of the statements *a* and *b* by the letter "*c*", it is necessary for us always to add, explicitly, whenever we mention *c*, that this *c* is a conjunction of the statements *a* and *b*. We can avoid this by introducing a new sign, such as "$\wedge(a, b)$"—which may be read: "the conjunction of the statement *a* and the statement *b*",—or, more briefly, "$a \wedge b$",—which may be read "(the statement) *a-and-b*", or simply "*a-and-b*". Using this method, D 3.01 becomes:

(D 3.01 +). The statement $a \wedge b$ is a conjunction of *a* and *b* if, and only if, $a \wedge b/a$, $a \wedge b/b$, and $a, b/a \wedge b$.

But this can be split up into two parts: a definition D 3.0 and three primitive rules of inference:

(D 3.0). "$a \wedge b$" is the (variable) name of the conjunction of the two statements *a* and *b* (called the two components of the conjunction). The three rules of inference for $a \wedge b$, are:

(3.1) $a \wedge b/a.$

(3.2) $a \wedge b/b.$

(3.3) $a, b/a \wedge b.$

It is clear that (D 3.0) is nothing but the introduction of a new label, and that only the three rules give the new label a meaning,—by relating it to deducibility.

A warning should be given here: as we have introduced it, *the symbol "$\wedge$" has no separate meaning at all.* It would be a mistake to consider it as an abbreviation of the word "and",—perhaps even as an alternative abbreviation to "*&*". We do not even assume that the language we are discussing—the language to which our statements *a*, *b*, *c*, . . . belong—possesses a special sign for linking statements into conjunctions. We can

easily imagine a language L, consisting, say, of simple subject-predicate statements, in which the end of a whole statement is always indicated by the word "STOP". In such a language, a conjunction of two or more statements may be expressed simply by omitting the word "STOP" between them, rather than by inserting a word corresponding to our "and". Furthermore, a language may dispose of different means for expressing conjunction. Our language may perhaps express conjunction not only by the omission of the word "STOP" but alternatively (taking a clue from Latin) by replacing the word "STOP" by the word "QUE"; and this may mean that the two preceding statements are intended to form a conjunction. (If three statements are to be linked, "QUEQUE" may conclude the third, etc.)

Any number of different methods of expressing conjunction may be invented, and may actually occur, even within one and the same language.

It is for this reason that we say that $a \wedge b$ is *a* conjunction of a and b, rather than that it is *the* conjunction of a and b. But however many conjunctions of a and b we may have in a language, they all must be equivalent if they satisfy our rules 3.1 to 3.3; for any two conjunctions of a and b which satisfy these rules (and those of section 2) can be shown to be mutually deducible.

The upshot of all this is that what we have defined is not so much the conjunction of a and b but the precise *logical force* (or the logical import) of any statement c that is equal in force to a conjunction of a and b. This may be made clearer, perhaps, by re-formulating our definition D 3.01 in this way:

(D 3.02). The statement c is equivalent (or substitutionally equal) to any conjunction of the statements a and b, if, and only if, c/a and c/b and a, b/c. Or in an alternative way of writing:

(D 3.03 +) $\quad c//a \wedge b \leftarrow\!\rightarrow (c/a \;\&\; c/b \;\&\; a, b/c).$

From this definition, we can obtain the three rules 3.1, 3.2, and 3.3 in the following way: if c and $a \wedge b$ are substitutionally equal, then: (1) $c/a \;\&\; c/b \;\&\; a, b/c$ (by force of D 3.02), and (2) we may substitute "$a \wedge b$" for "c" (by force of substitutional equality). But the result of this substitution leads precisely to the rules 3.1 to 3.3.

It will now be reasonably clear, I hope, what we mean if we say that we can define conjunction on the basis of the system of section 2, with "/" as our only specific logical term. It may be mentioned, in addition, that if we wished, we could easily proceed further, and define, with the help of our definition of a conjunction, the *sign of conjunction itself* (in English, the word "AND")

along the following lines. We could distinguish between preceding, intervening, and succeeding signs of conjunction (*i.e.*, signs preceding the two statements to be linked, or placed between them—like "AND"—or succeeding them—like "QUE" in our example above). Then we can define: A sign S of a language L is a preceding sign of conjunction in L if, and only if, for any pair of statements a and b of L, an expression consisting of S followed by a and by b is, in L a conjunction of a and b. Corresponding definitions would have to be laid down for intervening and succeeding signs of conjunction. Next, we could define: A language L contains a (preceding, etc.) sign of conjunction if, and only if, it contains a sign S such that S is a (preceding, etc.) sign of conjunction.

These definitions are intended to assume as little as possible about the particular method by which a particular language may express conjunction. But they are not applicable to *all* languages containing conjunction; they cannot achieve generality. It is always possible to design new methods of expressing conjunction which are not covered by any definition of this kind (for example, underlining in written language, intonation in spoken language, etc.); and I have mentioned this way of defining the term "sign of conjunction in L" merely in order to show that we do not, perhaps, pay for the generality of our approach by an incapacity of applying our theory to the usual types of language.

These comments upon the character of our definitions, and on the possibilities of extending them to the definition of certain signs of some language, apply, as indicated, not only to conjunction, but equally to disjunction, negation, hypotheticals, the quantifiers, etc., that is, to all *formative signs*[1] of a language.

We now turn back to the particular problems of conjunction. First we may mention that our definitions can be made much neater; for it so happens that the rules 3.1 to 3.3 can be replaced, in the presence of the rules of section 2, by either of the two following rules:

(3.4) $\quad a \wedge b/c \longleftrightarrow a,\ b/c.$

(3.5) $\quad c/a \wedge b \longleftrightarrow c/a\ \&\ c/b.$

[1] I prefer to speak of the "formative signs" of an object language where Carnap and Tarski speak of "logical signs". The reason is that I consider logic as a purely metalinguistic affair; on the other hand, the signs in question (of the object language) characterise the formal structure of that language.

Using 3.4, instead of using 3.1 to 3.3 (as we did in our definition D 3.03 +) we can replace D 3.03 + by the following definition:

(D 3.04+) $c//a \wedge b \leftrightarrow$ (for any statement $d : c/d \leftrightarrow a,\ b/d$).

Similarly, 3.5 would give rise to the following definition:

(D 3.05 +) $c//a \wedge b \leftrightarrow$ (for any statement $d : d/c \leftrightarrow d/a\ \&\ d/b$).

Each of these two definitions is adequate, on the basis of the system of section 2, but each of them presupposes (Tg), i.e. 2.5g, for adequacy.

But there is a method of generalising all these rules, by introducing many premises, $a_1, \ldots, a_n$, instead of one premise a. The various generalised rules look like this:

(3.1g) $a_1, \ldots, a_n,\ b \wedge c/b.$

(3.2g) $a_1, \ldots, a_n,\ b \wedge c/c.$

(3.3g) $a_1, \ldots, a_n,\ b,\ c/b \wedge c.$

(3.4g) $a_1, \ldots, a_n,\ b \wedge c/d \leftrightarrow a_1, \ldots, a_n,\ b,\ c/d.$

(3.5g) $a_1, \ldots, a_n/b \wedge c \leftrightarrow a_1, \ldots, a_n/b\ \&\ a_1, \ldots, a_n/c.$

On the basis provided by section 2, each of the last two rules,[1] 3.4g as well as 3.5g, turns out to be replaceable by the three rules 3.1g to 3.3g, and *vice versa*; but this result is in so far trivial as, on this basis, it also turns out that the generalisation is superfluous, that is to say, that we can everywhere omit the premises $a_1, \ldots, a_n$, which brings us back to our original forms 3.1 to 3.5. But this situation changes completely if we drop the generalised transitivity principle (Tg) and replace it by the simpler form (Te). In this case, all the rules 3.1 to 3.5 and 3.1g to 3.4g still follow from 3.5g; but the opposite is not the case.

Thus rule 3.5g is more powerful than the others; and indeed, it turns out that it allows us to dispense with (Tg); that is to say, it can be shown that, in the presence of 3.5g, (Tg) is secondary to (Te) and (Rg).

We shall briefly sketch the proof of this contention. The assumption of the generalised transitivity rule (Tg) is that the following m rules are valid:

$$a_1, \ldots, a_n/b_1\ \&\ a_1, \ldots, a_n/b_2\ \&\ a_1, \ldots, a_n/b_3\ \&\ \ldots\ \&\ a_1, \ldots, a_n/b_m \quad (1)$$

Now we obtain, from the first of these, *i.e.*, from

$$a_1, \ldots, a_n/b_1\ \&\ a_1, \ldots, a_n/b_2 \quad . \quad . \quad (2)$$

by rule 3.5g,

$$a_1, \ldots, a_n/b_1 \wedge b_2 \quad . \quad . \quad . \quad (3)$$

[1] Rule 3.4g has been suggested to me by Professor Bernays. This led me to adopt the simplified rule 3.4 (as rule 4.1 in section 4).

accordingly, we also have, by (1),

$$a_1, \ldots, a_n/b_1 \wedge b_2 \;\&\; a_1, \ldots, a_n/b_3 \quad . \quad . \quad (4)$$

and, applying rule 3.5g again, we obtain

$$a_1, \ldots, a_n/(b_1 \wedge b_2) \wedge b_3 \quad . \quad . \quad . \quad (5)$$

and so on, until all the various conclusions $b_1, \ldots, b_n$ are incorporated into *one*, as components of a conjunction. Now this process, which makes use only of 3.5g, allows us to replace (1) by one single rule of inference. To this single rule we can apply (Te), the extended form of the transitivity rule.

We may sum up our results as follows :

We have now two approaches for deducibility.

Approach I consists of the systems of section 2—in its simplest form, a basis consisting of two primitive rules : rule (Rg), the generalised principle of reflexivity, and rule 2.5*g* the generalised principle of transitivity, denoted by (Tg). We repeat these two rules, which constitute what we call the basis I :

(Rg) $\quad a_1, \ldots, a_n/a_i \quad (1 \leqslant i \leqslant n).$

(Tg) $\quad \prod_{i=1}^{m} a_1, \ldots, a_n/b_i \rightarrow (a_1, \ldots, a_n/c \rightarrow b_1, \ldots, b_m/c).$

This approach is applicable to any language.

Approach II is less generally applicable : it assumes that the language under consideration contains (in accordance with definition D 3.02) the operation of conjunction. In its simplest form, called " basis II ", it consists of a postulate (PC), expressing this assumption and replacing the definition D 3.02, and of three primitive rules, *viz.* of (Rg) and (Te), the simple form of the principle of transitivity previously called 2.042, and of 3.5g, which we shall call the generalised principle of conjunction. This is basis II :

(PC). If a is a statement and b is a statement, then $a \wedge b$ is a statement.

(Rg) $\quad a_1, \ldots, a_n/a_i \quad (1 \leqslant i \leqslant n).$

(Te) $\quad a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c).$

(Cg) $\quad a_1, \ldots, a_n/b \wedge c \longleftrightarrow (a_1, \ldots, a_n/b \;\&\; a_1, \ldots, a_n/c).$

These two approaches [1] are, of course, not equivalent ; basis II is richer than basis I, since it contains, for example, all the rules 3.1 to 3.5, and 3.1g to 3.5g (which basis I does not contain), and

[1] There are a number of alternative bases in each of the two approaches. I mention only basis I′, consisting of 2.1 ; 2.2 ; 2.3 ; (Tg) ; basis II′ uses only (Ts), together with 2.1 ; 2.2 ; 2.3 ; Cg, and either 3.4g or 2.8.

many other rules which may be derived from it; but they become equivalent in force if we add to basis I postulate (PC) together with, for example, the rules 3.1 to 3.3 (or with one of the rules 3.4 or 3.5, or else with one of the definitions D 3.03 to D 3.05).

4. The Logic of Compound Statements. Either of the two bases described in the foregoing sections suffices for the construction of modern logic,[1] *i.e.*, of the logic of compound statements [2] and of the logic of quantification (or the propositional and the lower functional calculus), and therefore *a fortiori* for constructing classical logic, so far as it is valid.

The logic of compound statements comprises the rules of inference pertaining to conjunction, to the hypothetical or conditional statement, its converse, and the bi-conditional, to disjunction, negation, etc. The system of these rules can be constructed by precisely the same method as was used in the previous section for introducing conjunction:

We first assume postulates (such as PC in section 3), one for each of the compounds we wish to introduce, in order to assure, for every pair of statements, say a and b, the existence of the corresponding compound statement. In this way, we may postulate for every pair of statements a and b the existence of a statement $a > b$ (if we want our language to contain hypothetical compounds); of a statement $a < b$ (if we want converse hypotheticals); of a statement $a \mathrel{\widehat{=}} b$ (if we want bi-conditionals); and of a statement $a \vee b$ (if we want disjunctions). Furthermore, we postulate that for every statement a, a statement $-a$ exists (if we want negations).

These postulates have the function of confining our investigation to languages in which such compounds exist. They can be replaced by definitions such as D 3.02; or alternatively we may formulate our theory hypothetically, by introducing an assertion about deducibility by a remark such as "If a language contains negation, then the following rules of inference hold: . . . ." In other words, the postulates or definitions do not really form a part of our theory of inference—they are there solely to indicate explicitly that the application of our theory is limited, if we

[1] I exclude the theory of membership (as Quine calls it) as mathematical. Classical syllogistic (so far as valid) appears as a small part of the logic of statement functions with one variable.

[2] The term "compound" is in so far misleading as we do not assume here that the so-called "components" are actually parts of the statements (the conjunction, etc.) whose components they are; see section 3. That is, we do not assume statement-composition, although our theory is applicable to it.

wish to operate with certain compounds, to languages which contain these compounds.

The meaning of the compounds will be determined by *primitive rules of derivation* (4.1 to 4.6), one primitive rule for each of the six compounds mentioned.

It may be asked whether any actually spoken language contains these compounds ? The answer, I believe, is : they do not ; but they contain them approximately. The actually spoken languages are a kind of natural growth, with rules of use which are not clearly defined, and which cannot be clearly defined ; if we define them in a definite way, then we replace the actually spoken and growing language by something else—by a kind of artificial reconstruction of it, or to put it bluntly, by a kind of artificial calculus.

This cannot be otherwise ; for there is no reason why all people who speak a certain naturally grown language should conform, in their habits of speech, to a precise system of rules of use. On the contrary, they will, at best, develop that degree of precision which is requisite to the ordinary problems of the day (rather than to scientific and, more especially, mathematical and logical problems). A grammarian who attempts to legislate for them in fact attempts to impose an artificial calculus upon them ; as a rule, he obtains his calculus by a process of idealising the speaking habits of what he considers to be well educated people. The logician also constructs an artificial model language or calculus, but as a rule (I hope) without attempting to impose it upon anybody.

Why does he do it ? As I indicated above, the fundamental problem of logic is the theory of inference—in the most elementary form, the drawing up of rules of valid inference. In this task, he cannot succeed without introducing some artificial precision into the language in which these inferences are formulated. For what he aims at is, at the very least, a distinction between valid and invalid inferences. But whether or not a certain concrete instance of an inference is valid will depend, very often,[1] upon the meaning of certain words which may occur in the premises or in the conclusion ; or more precisely, on the rules determining the use of these words. If, therefore, these rules are fluid, then the question whether or not a certain inference is valid cannot be answered.

[1] But not always ; for the theory of section 2 (or of our approach I) is independent of the meaning of any particular *word ;* all it presupposes is that we can identify *statements.*

Thus the logician cannot fulfil his most elementary task without constructing something like an artificial model language; and every logician from Aristotle onward has done so—although few were quite clear about what they were doing.

The primitive rules of derivation with the help of which we are about to determine the meaning of our compounds, in a way go beyond this method of constructing an artificial model language—they characterise a very wide class of (artificial) languages as conforming to certain definite and precise rules—rules which make it possible for us to develop a theory of inference.

Still, when all is said and done, it so happens that to each of our artificial compounds, there exists a closely corresponding compound proposition in English. The correspondence between the conjunction $a \wedge b$ and the English statements constructed with " and ", for example " It rains and it is wet ", are particularly close. But even the conditional statement $a > b$ corresponds very closely indeed to some current English expressions. The correspondence with a phrase such as " If it rains, then it is wet " or " Provided it rains, it is wet " is quite good, but that with the more complex statement " It does not rain without its being wet " is better. Accordingly, we can pronounce the symbol " $a > b$ " perhaps " if-*a*-then-*b* " or better " *a*-not-without-*b* ", remembering, however, that it is *not* an abbreviation for the English phrase but a *variable name* of any statement which stands in a certain logical relationship to the two statements a and b.

Similarly, we can read " $a < b$ " perhaps " *a-if-b* " or " *a-provided-b* ", and " $a \triangleq b$ " perhaps " *a-if-and-only-if-b* ".

The disjunction $a \vee b$ does not correspond badly to a phrase such as " It rains or it is wet or both ". The correspondence with the briefer phrase " It rains or it is wet " is very bad indeed, but we may for the sake of brevity pronounce " $a \vee b$ ", nevertheless " *a-or-b* ", having in mind that this is short for " *a-or-b-or-both* ".

There is no very fitting English phrase corresponding to the negation $-a$. The negation of the statement " It rains ", corresponds very well to " It does not rain " (rather than to the impossible phrase " Not it rains "), yet the negation of " All men are mortal " does not correspond to " All men are not mortal " but rather to " Not all men are mortal ". A generally applicable phrase which is not too bad in English and which corresponds very well to $-a$ is " It is not the case that . . . ". Abbreviating this, we can read " $-a$ " perhaps as " *not-a* ".

We now state our primitive rules :

(4.1) $$a \wedge b/c \longleftrightarrow a, b/c.$$

Or : " From the one premise *a-and-b* we can derive *c* if, and only if, from the two premises *a* and *b* we can derive *c*." (This is the same as 3.4. Note the trivial character of the rule. Nevertheless, it is important and very powerful in connexion with basis I ; with basis II it is, of course, redundant.)

(4.2) $$a/b > c \longleftrightarrow a, b/c.$$

Or : From the statement *a* we can derive the statement *b-not-without-c* if, and only if, we can derive from the two statements *a* and *b* the statement *c*. (Example : From " It rains incessantly every week-end " we can derive " Provided I frequently walk during week-ends, I frequently walk while it rains " if, and only if, we can derive from the two premises " It rains incessantly every week-end " and " I walk frequently during week-ends " the conclusion " I walk frequently while it rains ".)

(4.3) $$a/b < c \longleftrightarrow a, c/b.$$

(4.4) $$a/b \triangleq c \longleftrightarrow a, b/c \;\&\; a, c/b.$$

(4.5) $$a \vee b/c \longleftrightarrow a/c \;\&\; b/c.$$

(4.6) $$-a, b/-c \longleftrightarrow c, b/a.$$

Rules 4.1 to 4.5 constitute *the positive logic of compound statements*, that is to say, they suffice for that part of propositional logic which is independent of negation. If our rule 4.6 for negation is added, we obtain the whole of compound statements (or of propositional logic). If, instead, the two rules for quantification discussed in section 7 are added, we obtain the whole of positive logic.

Positive logic as defined by these rules does not yet contain all valid rules of inferences in which no use is made of negation : there is a further region which we may call the " extended positive logic " (or, if we exclude quantification, the " extended positive logic of compound statements "). This extension is achieved by adding to the system without 4.6 an additional rule for hypothetical statements, *viz.* :

(4.2e) $$a, b > c/b \longleftrightarrow a/b.$$

This rule may be, loosely, characterised as asserting that, if we add to a premise another one which merely informs us that something depends on our conclusion, then we do not thereby strengthen in any way the power of our original premise to yield that particular conclusion. An example is : From the two

premises " All men are mortal " and " If Plato is mortal then he cannot be a god ", we can derive the conclusion " Plato is mortal " if, and only if, we can derive this conclusion from the first premise alone.

If rule 4.2e is added as primary to our rules 4.1 to 4.5, then all valid rules of the logic of compounds in which negation does not occur become secondary rules. Also we do not need, in the presence of rule 4.2e, the whole force of our rule of negation 4.6, but can obtain this rule as a secondary rule from some weaker rules [1] (from the rules of the so-called intuitionist logic). On the other hand, if we have as primary the two rules 4.2 and 4.6, then 4.2e becomes a secondary rule.

Considering the triviality of these rules—especially of the " positive " rules 4.1 to 4.5—their power is truly amazing. To give a few examples:

From 4.1 alone (of course, the basis is always assumed), we can obtain as a secondary rule the associative law for conjunction

(4.01) $$(a \wedge b) \wedge c \,//\, a \wedge (b \wedge c);$$

furthermore, on the basis I, all the rules of section 3.

From 4.2 alone, we obtain, on either of the two bases, first of all

(4.020) $$a,\ a > b / b,$$

that is to say, the *modus ponens;* furthermore, the important rules

(4.021) $$a / b > a.$$

(4.022) $$a > (a > b) / a > b.$$

On the basis II, or if we add either rule 4.1 or 4.6, we obtain furthermore

(4.023) $$a > b / (b > c) > (a > c).$$

(4.024) $$a,\ b / c > d \longleftrightarrow a,\ b,\ c / d.$$

This last rule is a kind of generalisation of 4.2 which cannot, on the basis I, be obtained from 4.2 alone, but which can also be obtained with the help of 4.6 (instead of 4.1 or of basis II).

From the rule 4.024 alone, all the rules of positive logic containing no compound except hypotheticals are derivable without any other help,—of course, always assuming at least basis I.

[1] In fact, 4.6 assumes more than we need: we can replace in it " $\longleftrightarrow$ " by " $\rightarrow$ ", and still get 4.6 as secondary rule. It may be mentioned that our rule 4.6 amounts in effect only to a principle underlying the classical theory of " indirect reduction ".

Among these rules are

(4.025) $a > (b > c)/b > (a > c).$

(4.026) $a > (b > c)/(a > b) > (a > c).$

If 4.2e is added, we obtain *all* valid rules containing no compound other than hypotheticals. If we combine 4.1, 4.5 and 4.6, then we obtain, for example

(4.5) $a \wedge (b \vee c)//(a \wedge b) \vee (b \wedge c).$

From 4.6 alone we obtain, on either of the two bases, for example:

(4.061) $-a, b_1/a \rightarrow -a, b_1/b_2 \;\&\; b_1, b_2/a,$

a rule which, in its effect, is equal to 4.6; furthermore

(4.062) $a//--a.$

(4.063) $a, -a/b.$

(4.064) $a/b \;\&\; -a/b \rightarrow c/b.$

(4.065) $-c/b \rightarrow (a, b/c \longleftrightarrow a/c).$

These rules can hardly be called trivial; 4.063, for example, is a rule whose validity has been questioned in this journal not very long ago; 4.065 is, in spite of its comparative simplicity, hardly self-evident.

These examples are here given mainly with the intention of illustrating our thesis that we should not despise trivial primitive rules: they may give rise to secondary rules which are far from being trivial.

Regarding the question of the triviality of our primitive rules, it may further be observed that the rules 4.1 to 4.5, that is, the rules of the positive logic of compounds, are not only very obvious, but have, besides, the character of definitions (so-called contextual definitions) of the compounds which they introduce. The newly introduced symbol (the definiendum) occurs only once, on the left-hand side, while on the other side our specific logical term " / " is used, as defining term, apart from certain phrases of ordinary English, or their abbreviations.

The position is different with the rule of negation, 4.6. Here *two* negations occur on the left at the same time: the one is somehow linked up with the other, and can, therefore, not always be eliminated alone. Rule 4.061 is, in this respect, perhaps slightly preferable. But it has a very similar defect. The variable " a " occurs twice, and the rule is therefore not necessarily an effective means of eliminating the symbol " — " from any context. But a definition should be able to achieve this.

The peculiar situation of negation in this respect is connected, I believe, with the fact that various systems of logic have been developed which use a weaker form of negation (notably Heyting's intuitionistic logic which formalises Brouwer's views on logic). It is, therefore, of considerable interest to search for an explicit definition for negation which might permit us to eliminate it from any context, and which might bring it into line with the other compounds.

5. Explicit Definitions of the Compounds. It can be easily shown that the introduction of the compounds cannot in any way impair the substitutivity of logical equivalence. That is to say, whenever a and b are mutually deducible, we may substitute a for b, or b for a, at some or all places of occurrence, in any rule of inference, however complicated, without affecting the validity or invalidity of the rule. If we, therefore, use the method already exemplified by definitions D 3.03 + to D 3.05 +, then we cannot fail to obtain definitions which allow us to eliminate the defined sign without difficulty from any context.

All the definitions so obtained are rather obvious adaptations of the primitive rules of the last section, except the definition of negation which is based on 4.061 rather than on 4.6.

The Conjunction:

(D 5.1) $\quad a//b \wedge c \longleftrightarrow$ (for any $c_1 : a/c_1 \longleftrightarrow b, c/c_1$).

The Hypothetical:

(D 5.2) $\quad a//b \rangle c \longleftrightarrow$ (for any $a_1 : a_1/a \longleftrightarrow a_1, a/b$).

The Converse Hypothetical:

(D 5.3) $\quad a//b \quad c \longleftrightarrow$ (for any $a_1 : a_1/a \longleftrightarrow a_1, b/a$).

The Bi-Conditional:

(D 5.4) $\quad a//b \mathrel{\widehat{=}} c \longleftrightarrow$ (for any $a_1 : a_1/a \longleftrightarrow a_1, a/b \;\&\; a_1, b/a$).

The Disjunction:

(D 5.5) $\quad a//b \vee c \longleftrightarrow$ (for any $c_1 : a/c_1 \longleftrightarrow b/c_1 \;\&\; c/c_1$).

The Negation [1]:

(D 5.6) $\quad a//-b \longleftrightarrow$ (for any a_1 and $b_1 : a, a_1/b \rightarrow (a, a_1/b_1 \;\&\; \&\; a_1, b_1/b)$).

While none of these definitions use a defined term in the definiens, we may add some others which make use of defined terms:

[1] In this formula, the last occurrence of "b_1" may be omitted. It is added only in order to make a certain inherent symmetry (which is nothing but the symmetry between the laws of contradiction and excluded middle), more obvious. For an alternative definition, see the next footnote.

The Exclusive Disjunction (" *a-or-b-but not both* ") :

(D 5.7) $\quad a//a \dot{+} b \longleftrightarrow a// - (a \hat{=} b).$

The Alternative Denial (" *not-a-or-not-b* ")

(D 5.8) $\quad a//a \barwedge b \longleftrightarrow a// - (a \wedge b).$

The Conjoint Denial (" *not-a-and-not-b* ") :

(5.9) $\quad a//a \downarrow b \longleftrightarrow a// - (a \vee b).$

The Tautology :

(5.00) $\quad a//t(b) \longleftrightarrow a//b_{\vee} - b.$

The Contradiction :

(D 5.0) $\quad a//f(b) \longleftrightarrow a//b_{\wedge} - b.$

For the last two, we can easily prove the rules :

(5.001) $\quad t(a)//t(b).$

(5.01) $\quad f(a)//f(b).$

That is to say, all tautologies are substitutionally equal, and the same holds for all contradictions. This shows that we may omit as irrelevant the reference to " a " or " b ", and simply write " t " and " f " instead of " $t(a)$ " and " $f(a)$ ".

In a way, all these definitions are quite unnecessary. True, they may replace the primitive rules of section 4, but we can work just as well or better with the much simpler primitive rules. Nothing is added to our system or taken away if we replace the rules of section 4 by the definitions of the present section.

The only reason why we nevertheless present these definitions here is that they establish beyond any doubt the fact that our primitive rules of section 5 have the character of definitions, even the rule for negation.

This fact is, no doubt, interesting. It may help to clear up the discussion of the so-called " alternative systems of logic ". From our point of view, these systems are not alternative systems of logic, but alternative ways of using certain labels such as, for example, the label " negation ". For it is plain that, by using different primitive rules, or by using different definitions, we can define different concepts. There is no problem involved here. If we introduce instead of 4.6 a similar primitive rule like

(5.06_{M}) $\quad a, b/ - c \longleftrightarrow c, b/ - a,$

then this rule defines a concept similar to our concept of negation, as defined by 4.6 (the " classical " concept of negation), but also a little different. This concept may be called " negation_{M} " (it is, in the presence of the other rules except 4.6, identical with the

negation of Johansson's "Minimum Calculus"). It so happens that negation$_M$ is so closely related to our classical negation that all rules of inference based on negation$_M$ are also valid for classical negation, but not *vice versa*. (In this sense negation$_M$ can be said to be weaker than classical negation.) This is due to the simple fact that rule 5.06 is a valid secondary rule of our classical system. If we add to rule 5.06 another rule (which is not secondary to 5.06 but secondary to our classical system), *viz.*:

$$(5.06_I) \qquad -a, --a/a,$$

then these two rules together define another concept—we call it "negation$_I$"—the intuitionist concept of negation.

All these concepts can co-exist, in one and the same model language, as long as they are distinguished. Thus there is no quarrel between alternative systems. The rules of inference pertaining to the various concepts of negation are not identical, to be sure. But this is very satisfactorily explained by the simple fact that the various concepts have been given a meaning by rules which are not identical.[1]

The question whether ordinary spoken language uses the one or the other of these concepts does not, I believe, arise. Ordinary language is not sufficiently precise for us to ask such questions about it. As far as the somewhat more precise and therefore more artificial language of science is concerned, we are still asking too much, without doubt, if we ask such a question in connexion with it. But I suggest that there are good reasons why, for most purposes of science, the classical concept should be preferable to the others—simply because it is stronger, more explicit. This does not prevent us from using, for certain purposes, especially in parts of mathematics, the interesting concept negation$_I$ side by side with the classical one.

The upshot of all these considerations is this: If we have an artificial model language with signs for conjunction, the conditional . . . etc. (we have called them "formative signs" of the language in question) then the meaning of these formative signs can be exhaustively determined by the rules of inference in which the signs occur; this fact is established by defining our definitions of these formative signs explicitly in terms of rules of inference.

[1] We can use the following signs: "$\bar{\bar{c}}a$" for the classical negation of a, "$\bar{\bar{i}}a$" for the intuitionist negation of a. We can then introduce the following two explicit definitions, of which the first is a somewhat preferable alternative to (D 5.6):

(D 5.6c) $a//\bar{\bar{c}}b \leftrightarrow$ (for any b_1: $a, b/b_1$ & $(a, b_1/b \rightarrow b_1/b)$).

(D 5.6i) $a//\bar{\bar{i}}b \leftrightarrow$ (for any b_1: $a, b/b_1$ & $(b, b_1/a \rightarrow b_1/a)$).

There is good reason to believe that, in our ordinary language, we learn the intuitive meaning of formative signs in the same way; for no doubt, we learn their intuitive meaning by learning *how to use them*. In other words, we learn their meaning by learning the rules of their use, and these rules are mainly the rules of *their use in connexion with the drawing of inferences*.

Thus it is trivial that once we understand intuitively the meaning of formative signs, the proper handling of the rules of inference pertaining to them is part of this knowledge.

This is why rules of inference have that compelling character which mislead idealistic logicians to speak of laws of thought. Instead of taking this compulsion as the unanalysed and unanalysable *datum* of logic, we can indeed explain it quite easily. Once we know how to use the words "and", "provided", etc., that is to say, once we have learned the language and have got an intuitive grasp of its use, we feel compelled to admit that from *a* and *ab*, *b* can be deduced; in exactly the same way as we feel compelled to admit that this animal here is a cow (provided it is a cow), once we have learned how to use the word "cow", and have got an intuitive grasp of its use.

6. Statement-Functions. Substitution as a Logical Operation. We now extend the scope of our inquiry. So far, we have considered only one kind of expressions, namely statements. Now we are going to consider, in addition to statements, so-called "statement-functions".

An example of a statement-function is: "He is a charming fellow". Whether this is true or false will depend on the question whom we mean by "he", or in other words, whose name we substitute for the pronoun [1] "he"; whether we substitute "Ernest", say, or "Edward". The expression "he is a charming fellow" is as such neither true nor false, it is *no statement*, but it can be converted into a statement by a fitting substitution —of a name, or of a descriptive phrase such as "Ernest's best friend". Other examples are: "This is an ugly fireplace"; "He is his younger brother"; "He owes him the legacy left to him by his father". The last two examples show the need for making distinctions: they become clearer if we write, say, "He_1 is the younger brother of he_2", and "He_1 owes he_2 the legacy left to he_3 by the father of he_4"—of course, something else may be meant, for example, "He_1 owes he_2 the legacy left to he_3 by the father of he_1", or "He_1 owes to he_2 the legacy left to he_2 by the father of he_3", etc.

[1] The excellent suggestion to treat name-variables as pronouns is, so far as I know, due to Quine.

The need for a sufficiently long list of distinct pronouns is obvious, and we shall assume, of the languages under consideration, that they dispose of such a sufficient list. (See below, postulate PF3.)

Now consider again the last example. It is clear that we can *derive* from it the statement-function "He$_1$ owes to he$_2$ some legacy". Similarly, we can *derive* from the statement-function "He is not only a charming fellow but an excellent physician" the statement-function "He is an excellent physician". In other words, if our treatment of deducibility is to be as general as we intend it to be, then a treatment of the deducibility of statement-functions becomes necessary. At the same time, it will be necessary to treat combinations of statements and statement-functions.

We shall, in fact, assume that the theory of derivation developed in previous sections holds not only for statements but also for statement-functions.

Although statements and statement-functions should be clearly distinguished, they are, of course, rather similar things. A statement-function is, as it were, a statement with certain holes or openings in it, which can be filled up or closed up by inserting names. For this reason, statement-functions are often called "*open statements*", and statements proper are often called "*closed statements*". The terminology has great advantages, and we shall adopt it; not, however, without issuing a warning that the term "open statement" must not mislead anybody into believing that open statements are statements in the ordinary sense. They are not, for they are neither true nor false. "Open statement" is only an alternative, and often a more convenient, term for "statement-function". Only closed statements are statements proper.

Since we do not want to say that open statements are statements, we shall use another term—"formulæ"—to describe the class of expressions which consists of open statements as well as of closed statements. Thus we shall say that every open statement is a formula, and that every closed statement is a formula.

But apart from formulæ, we have another class of expressions to consider—the pronouns "he", "he$_1$", "he$_2$" . . . "it", etc. We shall call these pronouns the *name-variables* under consideration, since names may be substituted for them.

It is obvious that these two classes of expressions, *formulæ* and *pronouns or name-variables* have no member in common. This will be expressed in our postulate PF1.

Thus our universe of discourse will consist, from now on, of two entirely distinct classes of expressions—formulæ, *F*, and name-variables, *N*. We shall introduce the convention to use from now on "*a*"; "*b*"; "a_1"; "b_1" . . . as variable names of formulæ rather than as variable names of closed statements only (as before), and furthermore, to use "*x*"; "*y*"; "*z*"; "*u*"; "*w*"; etc., as variable names of pronouns. It so happens that "*x*"; "*y*"; etc., can be described as variable names of name-variables. This is perhaps a little confusing, for a moment or two, but quite in order.

The symbols "*x*"; "*y*"; etc., are variables which refer to certain symbols such as "he"; "he_1"; "it"; "it_1"; etc., of the languages under consideration. The symbols "he", "he_1", etc., refer in their turn to certain men—to Ernest, to Edward, etc. These *men*—not perhaps their *names*—constitute the "realm of individuals" or the "universe of individuals" to which the language *L* under consideration refers. This universe of the discourse of *L* is not the universe of *our* discourse; *we*, rather, discuss languages—*L*, for example—and especially certain classes of expressions.

The name-variables of *L* may occur in the formulæ of *L* in two ways: either as *free variables* or as *bound variables*.

In the open statements which we used as examples, all the variables occurred freely. But we can *bind* a freely occurring variable by placing before the formula in which it occurs the phrase

(A) "Whosoever he_1 may be . . . "

or the phrase

(E) "There exists at least one he_1 such that . . . "

The phrase (A) is called the "universal quantifier (binding he_1)", the phrase (E) is called "the existential quantifier (binding he_1)".

The theory of quantifiers will be sketched in the next section; in the present section, we deal merely with formulæ and variables in general. But it may be mentioned that, if all variables of a function are bound, the function is a closed statement, *i.e.*, a statement proper. Or in other words, an open statement may give rise to a closed one in either of the two following ways: by substituting names for the free variables, or by binding the free variables with the help of quantifiers. For example, the open statement "If he is a good physician, then he is a good man" may give rise to the statements:

(1) "If Ernest is a good physician then he is a good man."

(2) (A) "Whosoever he may be—if he is a good physician then

he is a good man." (This may be said to be the same as "All good physicians are good men": a universal statement.)

(2) (E) "There exists some he such that if he is a good physician then he is a good man." (This may be interpreted as: "There exists somebody who, if he is a good physician, is also a good man": an existential statement.)

We said before that the quantifiers may be placed before a function; but in another language (or in the same), they may be placed after the function, etc. In other words, the same problems arise as those we discussed in section 3 in connexion with conjunction, and the position of the sign "and". We shall, therefore, not assume that all the languages we wish to discuss contain distinguishable signs which we can call quantifiers. What we shall discuss, in the next section, is merely the way in which the formulæ, denoted by

"a"

and by

"Axa"

are related, if Axa is equivalent to the result of applying universal quantification (with regard to the variable x) to the formula a.

The sign "Ax" has therefore no more independent meaning for us than the sign "$\wedge$"; nevertheless, we shall loosely speak of "Ax" as of the name of a universal quantifier binding x, and of "Ex" as of the name of an existential quantifier binding x.

Leaving the quantifiers and the rules of inference which are to determine their meaning to the next section, we now turn to the theory of another and even more fundamental logical operation, also comparable to conjunction, *viz.*, the *operation of substituting the variable y for the variable x in the formula a*. The intuitive idea of substituting the variable "he_1" for the variable "he_2" in the formula "he_1 loved he_2" or "he_1 killed he_2", and the difference such a substitution should make to the meaning of a formula, seems to be perfectly clear. In fact, this idea seems to be so obvious that it is usually accepted as a kind of descriptive term that is not in need (and perhaps not capable of) logical analysis—least of all, of an analysis in terms of derivability. But the operation of substitution plays a very important part in quantification theory, and an attempt to reduce it to the concept of derivability seems highly desirable if we wish to build up a coherent theory.

For this purpose we first introduce the symbol "$a\binom{x}{y}$" as a convenient name for the formula which is the *result* of the substitution of the variable y for the variable x in the formula a, in

all places of the occurrence of x. (If x does not occur in a, then $a\binom{x}{y}$ is the same as a.) As in the case of the compounds, we are not going to characterise this result itself, but rather its *logical force—i.e.*, any formula which is equivalent to the result.

To this end, we first postulate, as a preparation, PF1, PF2, and PF3.

(PF1) If L is one of the languages under consideration, then the formulæ of L and the name-variables of L have no member in common.

(PF2) $a\binom{x}{y}$ is a formula (of L) if, and only if, a is a formula (of L), and x and y are name-variables (of L).

(PF3) If a is a formula then there exists at least one name-variable x such that $a//a\binom{x}{y}$ for any name-variable y.

The contents of the first two of these postulates are plain. The third has mainly the function of ensuring that, for every formula (especially for every open statement), there exists at least one variable which does not occur in it. (We mentioned before that, if x does not occur in a, then $a\binom{x}{y}$ is the same as a.)

We now proceed to the primitive rules of derivation which lay down the precise meaning in which the term "$a\binom{x}{y}$" is used within the theory of inference. These rules look complicated, but this is only due to their notation: their content is trivial indeed.

(6.1) $$a//b \to a\binom{x}{y}//b\binom{x}{y},$$

that is, in brief: equal substitutions in equal formulæ have equal results. This rule assures the continued substitutivity of equivalence in the theory of statement-functions and of quantification.

(6.2) $$a\binom{x}{x}//a.$$

This is plain.

(6.3) $$\text{If } x \neq y, \text{ then } \left(a\binom{x}{y}\right)\binom{x}{z}//a\binom{x}{y},$$

that is: once we have substituted y for x, then x no longer occurs (provided $y \neq x$), and further substitutions for x have, therefore, no effect.

(6.4) $$\left(a\binom{x}{y}\right)\binom{y}{z}//\left(a\binom{x}{z}\right)\binom{y}{z},$$

that is: if we substitute first y for x and then z for y, then the result is the same as if we had substituted at once z for x and, besides, z for y.

$$(6.5) \qquad \left(a\binom{x}{y}\right)\binom{z}{y} /\!/ \left(a\binom{z}{y}\right)\binom{x}{y},$$

that is, if we substitute for two variables x and z, the same variable y, then it is irrelevant which of these two substitutions we undertake first.

(6.6) If $w \neq x$; $x \neq u$; and $u \neq y$, then

$$\left(a\binom{x}{y}\right)\binom{u}{w} /\!/ \left(a\binom{u}{w}\right)\binom{x}{y}.$$

This says that, if we substitute for two *different* variables, x and u, two other variables, y and w, then it is irrelevant which of the two substitutions we undertake first—provided always that the variable to be substituted for x does not coincide with u, and *vice versa*.

These six primitive rules determine the meaning of the symbol "$a\binom{x}{y}$" in a way precisely analogous to the way in which, say, rules 3.1 to 3.3 determine the meaning of conjunction. And the meaning of "$a\binom{x}{y}$" is determined, precisely as was that of conjunction, with the help of the concept of derivability, "/".

To give a few examples of secondary rules: we can obtain first, from postulate PF3

(6.01) If, for any x and y, $a\binom{x}{y} /\!/ b\binom{x}{y}$, then $a /\!/ b$.

From this and 6.1, we obtain

$$(6.02) \qquad a /\!/ b \longleftrightarrow \left(\text{for any } x \text{ and } y : a\binom{x}{y} /\!/ b\binom{x}{y}\right).$$

Another example, secondary to 6.1; 6.2; 6.4, is:

$$(6.03) \qquad \left(a\binom{x}{y}\right)\binom{y}{x} /\!/ a\binom{y}{x}.$$

Quite important is the fact that, with the help of "$a\binom{x}{y}$", we can define a new concept—the non-occurrence of x in a, or, perhaps more precisely, the non-relevant occurrence of x in a (or the conception: "*a-does-not-depend-on-b*"). We denote this idea by "$a_{\grave{x}}$" (read: "*a-without-x*") and define it as follows:

$$(D\ 6.1) \qquad a /\!/ b_{\grave{x}} \longleftrightarrow \left(\text{for any } y : a /\!/ b\binom{x}{y}\right).$$

That is: a is equivalent to *b-without-x* if, and only if, a is equivalent to whatever may be the result of substituting in b for x.

If we neglect this concept then the result is, as a rule, that some formulæ can be proved in the systems in question which, in effect, amount to the assertion that there exists only *one* individual, in the realm of individuals to which the name-variables of L refer, that is in the universe of discourse of the language under consideration. This result is highly undesirable, and in order to make explicit that such a result is frankly contradictory to our intentions, I suggest including in our list of postulates for the logic of functions the following:

(PF4) In every language under consideration, there exists at least one a, one x, and one y, such that

$$a/a\binom{x}{y} \rightarrow t/f.$$

Since "t/f" can be shown to be contradictory, this postulate has the result that a theory in which the rule

$$a/a\binom{x}{y}. \quad . \quad . \quad . \quad . \quad (1)$$

or even

$$a\breve{y}/a\breve{y}\binom{x}{y} \quad . \quad . \quad . \quad . \quad (2)$$

holds becomes frankly contradictory. But these rules (1) or (2) are just the rules asserting in effect that there exists only *one* individual. For they assert that *all* formulæ such as "he_1 loves he_2" are equivalent to "he_1 loves he_1"; and this can be so only if "he_1 is not identical with he_2" is equivalent to "he_1 is not identical with he_1", which is only the case if there exists only one he.

This indicates that we may be able to express *identity and difference of individuals* with the help of the means at our disposal; and although we cannot, of course, define the sign of identity in L (there may be no such sign in L), we can, again, as in the case of conjunction, define the logical force of any statement-function expressing identity between the variables x and y. If we denote such a function by "$Idt(x, y)$" we can define:[1]

[1] To be exact, we have, of course, to add the following postulate as a preliminary to the definition. (A similar postulate would have to be added for difference.)

(P Idt) If x and y are name variables, then $Idt(x, y)$ is a formula.

Furthermore, our defining rules of substitution should be extended by introducing such rules as:

(A) $$(Idt(x, y))\binom{x}{z} // Idt(z, y).$$

[*Continuation overleaf.*

(D 6.2) $a//Idt(x, y) \leftrightarrow$ (for every $b : b/a\binom{x}{y}$
$\& \; a, b\binom{z}{x}/b\binom{z}{y} \; \& \; a, b\binom{z}{y}/b\binom{z}{x}$).

Difference is easy to define:

(D 6.3) $a//Dff(x, y) \leftrightarrow a// - Idt(x, y)$.

This shows that these two concepts are logical concepts, and that the corresponding signs—if any—of the languages under consideration are formative signs.

7. *Quantification.* The theory of quantification is too subtle to be analysed in detail within the framework of this paper. All I shall attempt is to show that our two rules, 7.1 and 7.2, are not more complicated than previous ones, and that they can be considered as *definitions of universal and existential quantifications.*

Our two primitive rules are [1]

(7.1) If $b\binom{x}{y}$ is a formula, then:

$$a\grave{y}/Axb\grave{y} \leftrightarrow a\grave{y}/b\grave{y}\binom{x}{y}$$

or in a different way of writing:

If a and $b\binom{x}{y}$ are formulæ, then:

$$a/Axb \leftrightarrow a/b\binom{x}{y} \qquad (a{=}a\grave{y}\,;\; b{=}b\grave{y}\,;\; x{\neq}y)$$

(7.2) If $a\binom{x}{y}$ and b are formulæ, then:

$$Exa\grave{y}/b\grave{y} \leftrightarrow a\grave{y}\binom{x}{y}/b\grave{y}$$

or in alternative formulation:

$$Exa/b \leftrightarrow a\binom{x}{y}/b \qquad (a{=}a\grave{y}\,;\; b{=}b\grave{y}\,;\; x{\neq}y).$$

If we wish, we can transform these rules into explicit definitions:

(D 7.1) $a//Axb\grave{y} \leftrightarrow \left(\text{for every } c\grave{y} : c\grave{y}/a \leftrightarrow c\grave{y}/a\binom{x}{y}\right)$.

(B) $(Idt\ (x, y))\binom{y}{z}//Idt(x, z)$.

(C) If $x = u = y$, then $Idt(x, y))\binom{u}{z}// \; Idt(x, y)$.

[1] Postulates and rules of substitution have also to be assumed, corresponding to those mentioned in the last footnote.

(D 7.2) $a//Exb\hat{y} \longleftrightarrow \left(\text{for every } c\hat{y} : a/c\hat{y} \longleftrightarrow a\binom{x}{y}/c\hat{y}\right).$

The definitions are, as in previous cases, no improvement—they only establish the fact that quantification can be defined directly on the basis provided by sections 2 or 3, and the primitive rules 6.1 to 6.6.

I do not claim that the two new primitive rules are trivial in the sense in which our basic rules of sections 2 and 3, or even the rules of section 4, are trivial. There is, without doubt, a certain subtlety involved in the way these rules make use of the concepts "$a\binom{x}{y}$" and "$a\hat{x}$" defined in section 6,—a subtlety which defies a simple explanation Yet anybody who is conversant with quantification theory will, I am sure, be astonished that rules of this comparative degree of simplicity—and, for those who are accustomed to this kind of thing, even triviality—are sufficient. For the hitherto known comparable systems of quantification theory are much more complicated. For example, the well-known system of Hilbert and Ackermann contains (in place of our two rules) two axioms; two rules rather similar to our own; four very complicated rules for the handling of variables; and at least one rule forbidding certain operations (which are permitted in our system). All these rules, except the last mentioned, can be shown to be secondary to our two rules. Or to take another system—the very interesting and suggestive one of Quine's *Mathematical Logic*. This system takes only the universal quantifier as primitive. (The existential quantifier is introduced by a well-known definition with the help of the universal quantifier and negation.) Quine's system corresponds therefore to our *one* rule 7.1. But Quine needs five axioms—partly rather complicated ones—which can, of course, all be shown to be secondary to our rule 7.1.

Among the secondary rules which can be shown to be valid in every adequate theory of quantification, there are extremely complicated ones—here logic definitely ceases to be trivial. But it is our contention that these complications are merely due to the wealth of possible iterations and combinations of essentially trivial rules. For even if our latest defining rules are no longer very trivial, they are only definitions, that is to say, nothing but abbreviations; and they all go back to one specific logical concept—the concept of derivability, " / ", introduced by the undoubtedly trivial rules of sections 2 and 3.

8. Derivation and Demonstration. If we omit from our system the definitions of tautology and contradiction, D 5.0 and D 5.00, then our whole system is one of *purely derivational logic.* By this we mean the following. Take any language in which ordinary descriptive statements occur, such as, in English, statements like "If it is raining then the streets are slippery" whose truth or falsity cannot be decided by merely logical considerations (in contra-distinction to the tautology "If it is raining then it is raining"). Call these descriptive statements "non-logical statements". Then we can define: (D 8.1) A system of primitive rules of derivation is called "purely derivational" if, and only if, we can give such examples for each primitive rule that all of the statements (whether components or compounds) which occur in the examples are non-logical.

The point of a purely derivational logic is this: it is a system intended from the start to be a theory of inference in the sense that it allows us to derive from certain informative (non-logical) statements other informative statements.

Most systems of modern logic are not purely derivational, and some (for example in the case of Hilbert Ackermann) are not derivational at all. They operate not so much with rules of inference as with axioms or with rules of proof (axiom schemata). That is to say, they take as primitive such assertions as "All statements designated by "$a > a$" are true" (or "are provable" or "follow from the empty class of premises", etc.; these are just so many ways of stating axiom schemata).

These procedures are in themselves unexceptionable. But they are liable to blur the distinction between derivation and proof; they are liable to create the impression that every logical proof is a derivation whose first premises are logical axioms.

But this impression is definitely wrong. It is true that a derivation whose premises are logical axioms is a proof. But this is so only (1) because every derivation whose premises are provable is a proof, and (2) because the so-called logical axioms are in fact provable.

Let us consider for a moment one of the intuitive ways which we use in proofs,—say in an indirect proof.

Assume that we succeed in deriving, from one premise a

$$a/b \quad . \quad . \quad . \quad . \quad . \quad (1)$$
$$a/-b \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Then we argue in this way: a must be absurd, for from it follows b as well as $-b$, and therefore $b \wedge -b$, which is certainly false.

Thus a is *logically refuted;* but its negation, $-a$, must be true. Thus we have proved $-a$.

If we look at this argument, then we see that it refers to two derivations, (1) and (2); and that it argues from derivability to refutability and demonstrability.

In this way, *all proofs refer to derivations;* the derivations are the most conspicuous parts in proofs; but the derivation is not the proof.

Take another example.

If we find that the following derivations are valid

$$a/b \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$
$$-a/b \quad . \quad . \quad . \quad . \quad . \quad (2)$$

then we say that we have proved b. For we say, one of the two premises, a or $-a$, must be true; and if both the derivations are valid, the truth of the true premise must be transmitted to the conclusion. Thus b must be true—we have proved b.

Now what these proof schemata have in common is this: They use derivation.

But while *derivation* only establishes that the conclusion is true *provided* that the premises are true, the proof seeks to establish the truth of its conclusion independently of the question whether the premises are true or false. It tries to establish the truth of the conclusion unconditionally.

But is such a thing possible? Yes, it is possible, although only for comparatively rather uninformative statements—for what we may call "logical truisms".

We can now give a definition of a logical proof or demonstration:

(D 8.2) The statement a is demonstrable $\longleftrightarrow$ the statement a can be validly derived from any premise whatsoever.

This means, indeed, that the statement can be shown, by means of derivations, to be true unconditionally—independently of any particular premise. (If we derive it from b and doubt the truth of b, we can at once derive it from $-b$ also, or from c, etc.)

We shall write "$\nmid a$"

for "the statement a is demonstrable". Then D 8.2 becomes:

(D 8.2 +) $\nmid a \longleftrightarrow$ (for any statement $b : b/a$);

we can, similarly, define refutability, writing

"$\mathbf{7}\, a$"

for "a is refutable":

(D 8.3) $\mathbf{7}\, a \longleftrightarrow$ (for any statement $b : a/b$).

Now on the basis of our purely derivational logic, it is possible to show strictly:

(8.1) All the so-called axioms of the logic of propositions and propositional functions are provable; for example, those of *Principia Mathematica*, etc.

(8.2) Every statement that can be validly derived from a demonstrable statement is demonstrable, *i.e.*,

$$(\vdash a \;\&\; a/b) \to \vdash b.$$

(8.3) If we add demonstrable statements to the premises of any derivation, then the validity or invalidity of the derivation remains unaffected.

All these are principles which have been more or less intuitively used so far, but which can be strictly established as consequences of our definition of proof.

But there are further results. For example we can easily show

(8.4) $$a/b \to (\vdash a \to \vdash b),$$

which is only another form of 8.2; *but at the same time, the converse is not true*, that is to say, we must be careful not to assert that

$$(\vdash a \to \vdash b) \to a/b.$$

(Take a to be non-logical and b to be contradictory: this is a counter example.)

This warning is very important, for there are valid *rules of proof* of the theory of statement-functions such that

(8.5) $$\vdash a_{\grave{y}} \longleftrightarrow \vdash a_{\grave{y}}\binom{x}{y}$$

for which the corresponding *rule of derivation*

$$a_{\grave{y}}/a_{\grave{y}}\binom{x}{y}$$

is invalid.[1] Among the valid rules of proof are the important rules

(8.9) $$a_{\grave{x}}/b \to a_{\grave{x}}/Axb.$$

(8.91) $$a_{\grave{y}}/b_{\grave{y}}\binom{x}{y} \to a_{\grave{y}}/Axb.$$

Now these—for example 8.9—may be transformed in this way:

(8.92) $$\vdash a_{\grave{x}} > b \to \vdash a_{\grave{x}} > Axb.$$

But they must not be transformed in this way:[2]

$$a_{\grave{x}} > b/a_{\grave{x}} > Axb,$$

[1] *Cp.*, as opposed to this, Carnap, *Formalisation of Logic* (1943), p. 137, D. 28-2, rule 10.

[2] *Cp.* Carnap, *ibid.*, p. 138, rule 11; this is undoubtedly suggested by Hilbert-Bernays' way of writing these rules in the same style as the *modus ponens* (*cp.* pp. 105 f.) which suggests that they are rules of derivation. Hilbert Ackermann, on the other hand, write p. 57 the *modus ponens* as if it were a conditional rule of proof.

because otherwise

$$\vdash a > Axa$$

becomes provable, and with it,

$$a/|a\binom{x}{y}$$

could be shown to be valid—which violates our postulate PF2, of section 6.

Now all the mistakes here warned against do actually vitiate some otherwise very excellent books on modern logic—an indication that the distinction between (conditional) rules of proof or rules of demonstration on the one side and rules of derivation on the other cannot be neglected without involving oneself in contradictions.

It may be mentioned, in passing, that derivational logic *must* operate with a theory of open sentence functions, while demonstrational logic may neglect this part of logic.

We may perhaps sum up by saying that a purely derivational logic can get over an awkward dualism which demonstrational logic cannot avoid. Demonstrational logic must start with axioms—at least one—and rules of inferences—at least something like the *modus ponens*. As opposed to this, derivational logic operates monistically, *i.e.*, with rules of inference alone. The so-called axioms of the various systems of the logic of demonstration can be proved without assuming one of them.

9. Metalanguage and Object Language. Why do we study logic? We all understand our language, which means that we all know how to draw inferences, as we have seen. We do not need logical theory for being able to draw inferences; nor does the study of logical theory lead to a method of making all inferences fool-proof.

There are two obvious reasons for studying logic. One is curiosity, theoretical interest in languages and into their rules of use. This needs no apology. The other is practical. Our naturally grown languages have not been designed for the use to which we put them in scientific and mathematical investigations. They definitely do not always stand up too well to the strain of modern civilisation: paradoxes arise, spurious theories; the conclusiveness of some of our subtle arguments becomes doubtful. Such practical needs lead us to study the rules of language in order to design an instrument fit for use in science.

But for the study of language, we have to *use* language; and we have to use it freely and fully, without being afraid of using it. There is no study of any object whatever without the full

use of language if we need it. As the zoologist uses language to describe the behaviour of reptiles or lions, so the student of language must use language in order to describe the behaviour of statements, formulæ, variables, etc.

But is there not a danger that our studies may be vitiated if we use and study language at the same time? Undoubtedly there is. But there exists a simple device which seems to get over these difficulties beautifully. It is, simply, always to distinguish the language which is the object of our studies (the "object language") from the language we are using. The latter, if used for the discussion of other language, is called the "meta-language".

The fundamental idea of the distinction between object language and metalanguage is this: we simply cannot use *and* discuss one and the same language at the same time. Thus we decide to acknowledge the impossibility, and *we do not worry too much about the language we are using*—as long as we are using it. If we feel doubtful whether our way of using it was quite in order, then nothing will prevent us from proceeding, later, to an investigation of our metalanguage, using thereby what has been called a "meta-metalanguage" (etc., as long as we like to go on).

I mentioned that we should use the metalanguage freely. By this I mean that we must not be deterred, for example, from using conjunction in the metalanguage when defining conjunction in the object language. We *must* speak, if we wish to define. It is impossible to speak properly in our language of ordinary use without using conjunction, the conditional, and indeed all its formative signs. Here is no vicious circularity: we do not attempt to define the words we are using, but we use words in order to define very general concepts referring to other languages.

The demand that, in order to avoid circularity, we should avoid, in defining signs of the object language, the use of formative signs in the metalanguage, is no better than the impossible demand that we should avoid the use of statements in the metalanguage; for in most languages we can have no statements at all if we avoid formative signs. Incidentally we can, in our definitions, avoid even the appearance of circularity —we can for example define negation without making use of negation in the metalanguage, and we can define "$a \wedge b$" without using "&" in the metalanguage, and "$a \vee b$" without using "or".

As to our own investigations, we find, if we proceed to study our metalanguage in the meta-metalanguage, that our meta-

language can be easily formalised. It turns out that we do not need more than positive logic, including quantifiers (if we exclude the postulate PF4 which need not be assumed). Thus it happened that we defined negation, without using it in the metalanguage (incidentally and without intending it). This fact may be interesting for some reason or other; but had we to make use of negation in the metalanguage, in order to define what we mean by negation in the various object languages we were then studying, there would have been no circularity involved.

II.—THE CONTRARY-TO-FACT CONDITIONAL.

By Frederick L. Will.

In his article on the above subject in Mind of October, 1946, Roderick M. Chisholm has noted in some detail how the truth-functional "if-then" of the ordinary two-valued logic, such as that of *Principia Mathematica*, diverges strikingly in meaning from some of the important usages of the phrase, "if-then", especially as this phrase is employed to affirm in conditional statements what are held to be true connexions between antecedents which are false and their consequents, or between a class A which is known to be an empty class and some other class B. "If George had been at the meeting, he would have been embarrassed" is a typical case in point. Although George was not at the meeting, as the use of the subjunctive here implies, and hence the statement "George was at the meeting" is false, one wants to assert what would have been a consequence if the statement had been true and George had been present. One wants to assert that in that case it would be true that George was embarrassed and, consequently, that it would be false that he was not embarrassed. The substitution of the truth-functional connective "$\supset$" for the "if-then" in a statement of this kind does not provide such an assertion. The rules of statement-composition concerning the symbol "$\supset$" permit one to derive from $\sim p$ both $p \supset q$ and $p \supset \sim q$. Hence, if it is true that George was not at the meeting, both the following statements are true: (1) "(George was at the meeting) $\supset$ (George was embarrassed)", and (2) "(George was at the meeting) $\supset$ (George was not embarrassed)". These two true statements are clearly far from being equivalent to the two corresponding contrary-to-fact conditionals, which are themselves logical contraries: (3) "If George had been at the meeting, he would have been embarrassed", and (4) "If George had been at the meeting, he would not have been embarrassed".

The employment of the same symbol in the functional calculus produces similar discrepancies from the accepted usages of "if-then" in affirming relations between classes, and leads to the consequence which may be expressed informally in the principle that a null class is included in any class. Because of this the substitution of this symbol for the phrase "if-then"

does not provide an adequate translation of statements in which one wishes to affirm what would be as a matter of fact a consequence of being an instance of a given class, although that class does not indeed have any instances. In relation to Bradley's example of the trespassers one might wish to affirm that, although there were no trespassers, if there had been any they *would* have been prosecuted, and *would not* have gone unprosecuted. But if it is true that there were no trespassers, then the corresponding functional conditionals constructed with " $\supset$ " are not contraries but are as a matter of fact both true. It is true that (x) (x was a trespasser $\supset$ x was prosecuted), and also that (x) (x was a trespasser $\supset$ x was not prosecuted).

As a means to a more adequate translation of subjunctive or contrary-to-fact conditionals Chisholm proposes, following some suggestions of F. P. Ramsay and W. V. Quine, that they be interpreted as affirming a strong relation between statements such as that of logical implication or entailment. This relation would not be affirmed to hold between the antecedent and the consequent, for that would be to interpret every such conditional as a purely logical statement, and it would follow then that every true contrary-to-fact statement is analytic. The proposal is rather to translate each subjunctive or contrary-to-fact conditional which is not analytic into an equivalent statement which affirms "that the consequent is entailed by the antecedent taken in conjunction with a previous stock of knowledge" (pp. 298-299). For the conditional "If Holbrook were elected, the price of wheat would rise" he proposes the following mode of translation. Let H be the indicative version of the original antecedent, *i.e.* "Holbrook is elected"; and let W be the indicative version of the consequent, *i.e.*, "The price of wheat will rise". Then the original conditional can be regarded as another way of saying, "There is a statement p, such that p is true, and p and H entail W".

There are, however, as Chisholm quickly points out, certain ways in which this mode of translation may be inadequate unless some restrictions are placed upon it. For supposing that this is a way of translating contrary-to-fact conditionals which will permit them to be expressed adequately in a modern extensional logic containing the ordinary truth-functional modes of statement-construction, it can easily be seen that the same characteristic which permits $p \supset q$ to be derived from $\sim p$, or $\sim p \vee q$ to be derived from q, will permit one to construct statements to be substituted for p in the formula of translation which will permit one to derive a contrary-to-fact conditional to the effect that if p were true, q would be true, solely on the basis of some

evidence which establishes that p is false or that q is true. Consider again the translation of the conditional concerning the effect of Holbrook's election. It is, "There is a statement p, such that p is true, and p and H entail W". And suppose that it is true that Holbrook will never be elected to public office. Then solely on the basis of this truth it is possible to derive in the ordinary extensional logic, a statement which satisfies the requirement of p in the formula of translation. That statement might be, "(x) (x is a public office) · (Holbrook is elected to x) . $\supset$. (the price of wheat will rise)". This statement is true, and it and H entail W. It is therefore possible in this manner, if the formula of translation is accepted without qualification, to derive as a true conditional "If Holbrook were elected, the price of wheat would rise" solely from the true statement that Holbrook will never be elected to any public office. And from the same true statement it is possible to derive true conditionals affirming that if Holbrook were elected this year's wheat would turn to gold, Stalin would join the Catholic Church, or that any other consequence would ensue which one pleases.

It is this process which Chisholm refers to as trivialisation. It is the process by which it is possible, through the use of the truth-functional mode of statement-construction of the ordinary two-valued logic, to construct for the sentence p in the formula of translation expressions which have the effect of permitting one to derive as true contrary-to-fact or subjunctive conditionals the same type of trivial or vacuously true statements which are derivable when the symbol "$\supset$" is employed generally as the equivalent of "if-then". So long as this process is possible, the project of securing a mode of translation which will yield, in a logic involving the ordinary truth-functional mode of sentence-composition, statements which will be equivalent to the contrary-to-fact conditionals of ordinary language, has not succeeded. The principle by which this comes about is most easily seen in dealing with the simple logic of statement-construction systematised in the sentential calculus. In accordance with the rules of this logic, whenever q is assertible as true, $q \vee p$ is likewise assertible as true. Now it is a law of the logic of entailment that, for any p and any q, $q \vee p$ and $\sim p$ together entail q.[1] It is therefore possible, unless some restrictions are put on the character of the p which is asserted to be true in the formula of translation, to construct a p satisfying the formula in every case

[1] In the system of strict implication, Lewis and Langford, *Symbolic Logic*, this is directly derivable from 14·29, $p \cdot p \supset q :\supset . q$.

in which it is possible to affirm a truth-functional statement of the form $p \supset q$, and the formula will be just as broad and in this respect just as inadequate as the simpler formula of translation by substituting " $\supset$ " generally for every instance of " if-then ".[1]

In order to prevent this trivialisation of the formula for the translation of subjunctive and contrary-to-fact conditionals Chisholm points out generally the kind of restrictions which must be put upon p; and after illustrating the need for and effect of these restrictions in several examples, he formulates them as they apply to the specific form of conditional with which he is most interested in his paper. That form is: $(x)\ (y)$ if x were a ϕ and y were a ψ, then y would be a χ. The restrictions are stated in the following syntactic form:

> p includes no propositional function having free variables other than x and y which is not either a universal conditional or an existential statement; p includes no universal conditional which does not have existential import; and p includes no universal conditional whose consequent includes any two functions which are logically equivalent to functions having 'x is ϕ' and 'y is ψ' and 'y is χ' as corresponding instances, or whose antecedent includes any function not containing the variable of quantification.[2]

And to these qualifications he adds, as a means of precluding trivialisation in cases where the consequent of the subjunctive conditional happens to be true, that the indicative version of the consequent does not entail p.[3]

The number and complexity of the restrictions here stipulated for one fairly simple form of subjunctive or contrary-to-fact conditional does not encourage one to take a favourable view of the feasibility of providing, by means of the formula and restrictions upon its use of the above syntactic type, a general set of transformation rules which will be an efficient instrument for the translation of such conditionals, as they abound in science and in other areas of human experience, into exactly equivalent

[1] *E.g.*, let s_1 be any statement and let s_2 be any true statement. Then $s_1 \vee \sim s_1$ is true, and because it is, the subjunctive conditional which is semantically equivalent to " If s_1 were true, then s_2 would be true " is itself true. This result, it is clear, does not depend in any way upon the use of the symbol " $\supset$ " as the equivalent of " if-then ", for the symbol " $\supset$ " does not appear in p or anywhere else. The consequence here demonstrated is the result of the general truth-functional mode of statement-construction and would follow even though the extensional language in which the translation of the contrary-to-fact conditionals is to be effected were one which contained neither the phrase " if-then " nor any symbol which is synonymous with " $\supset$ " as it is ordinarily defined.

[2] Pp. 300-301. [3] P. 301.

statements constructed according to the truth-functional rules of sentence construction. There is no doubt that if such an instrument were procurable it would be useful. On the other hand, there is no reason to regard it as indispensable. There is now sufficient evidence in the logic itself, as well as in the manifold cases of its successful employment, that the truth-functional logic can be employed usefully as an instrument for the elaboration and appraisal of deductive inferences, and furthermore, that this useful employment does not require one to view the logic as defining a substitute language into which all the statements with which the deduction is concerned can be exactly translated. In view of the number and detail of the syntactic restrictions with which Chisholm has surrounded the employment of the formula in the translation of the above form of subjunctive or contrary-to-fact conditional, this is fortunate. And it is the more fortunate since it can be demonstrated without difficulty that these restrictions, as they stand, are not sufficient to prevent the trivialisation of the formula in relation to subjunctives of this form. If this trivialisation is to be avoided by such syntactic restrictions, the restrictions will have to be more numerous and detailed than they already are. And this is true in spite of the fact that Chisholm's last restriction, as will be shown immediately, is dispensable.

In what follows the symbols A and C will be employed in dealing with applications of the formula as abbreviations for the antecedent and the consequent respectively in any case in which the formula is satisfied, *i.e.*, in which there is a p such that p is true and p and A entail C.

Item 1. Chisholm's restriction, $\sim$ (C entails p), is dispensable and should be replaced by a converse restriction, $\sim$ (p entails C), which is itself indispensable in the sense that, whether or not the former restriction $\sim$ (C entails p) is included, there are forms of trivialisation which are possible so long as the latter restriction is not satisfied.

In making the former restriction, that the indicative version of the original consequent does not entail p, Chisholm does not explain how he conceives trivialisation to be possible when p is so entailed, except to indicate that these are cases of trivialisation which arise when the consequent happens to be true. When C is true, and the formula is used without some kind of restriction on the relations of logical dependence which may hold between C and p, trivialisation of the following kind can be affected. Let s_2 be any true statement and let s_1 be any statement. Since s_2 entails s_2, and s_2 is true, the formula of translation is satisfied.

There is a p (*viz.* s_2) such that p is true and p and s_1 entail s_2; and s_1, as stipulated above, can be any statement whatever. Therefore if the formula is used in this unrestricted manner, it enables one to derive from the known truth of any statement vacuous subjunctive conditionals of exactly the same kind as are produced by the general rule of using " $\supset$ " as synonymous with "if-then".

It is to be noted that in the case considered above the truth of both C and "C entails p" generates trivialisation because C and p are equivalent, *i.e.*, they entail each other. In that case, since p is true and itself entails C, the formula is satisfied no matter what statement appears as A, and that means that an indefinitely large set of subjunctive conditionals can be derived from the formula, all having the form "if A were true, then C would be true", and A can be any statement one pleases. On the other hand, if C is true and p is entailed by C but does not entail C, these facts alone will give rise to no trivialisation. Under these circumstances p is true, is entailed by C, and p and A when taken together, but not p separately, entail C. From these assumed facts concerning C and A, taken by themselves, no trivial contrary-to-fact conditionals can be derived.[1] Since p by itself does not entail C, A cannot be any statement whatever. For the satisfaction of the formula it is required that A be the kind of statement which, when combined with p, will entail C. By itself p is a necessary but not sufficient condition of the truth of C; p and A together constitute a sufficient condition of the truth of C which in some cases (when C likewise entails A) is a necessary condition and in some cases is not.

Since in every case where the conditions of the formula are satisfied and the condition that C entails p is also satisfied, but p does not entail C, A is not superfluous, but determined in

[1] Although trivial conditionals of the kind discussed are not derivable solely from the combined truth of C and "C entails p", this does not mean that the combined truth of these statements does not permit, under additional conditions, as in the case where p also entails C, the derivation of such statements. And it also permits, under some other conditions, the derivation of conditionals which in a different way reveal the deficiencies of the formula, when employed without restrictions, as a means of translation. *E.g.*, let C be the following statement, and suppose that it is known to be true, "Dewey will be nominated for President by the 1948 Republican National Convention and will be the next President of the U.S.", and let p be "Dewey will be the next President of the U.S.", and let A be "Dewey will be nominated for President by the 1948 Republican National Convention but will decline the nomination and forthwith announce his membership in the Communist Party".

character by the requirement that the conjunction formed by it and p entails C, the stipulation $\sim$ (C entails p) is too restrictive.[1] For this reason trivialisation derivable when C entails p is better prevented by other stipulations, as, for example, $\sim$ (p entails C). This stipulation is less severe than its converse, and without it, even though the former restriction $\sim$ (C entails p) is retained, a vast class of trivial statements of a very simple kind are derivable which must be suppressed if trivialisation is going to be excluded by the rules of translation. For if p is permitted to entail C in the employment of the formula, in any such case A is clearly superfluous; and an indefinitely large set of trivial statements to the effect that if A were true, C would be true are derivable by means of the formula, and A can be any statement one wishes. The restriction that p does not entail the indicative version of the consequent, or some equivalent restriction, is thus necessary, while the former restriction, that p is not entailed by C, is too severe and should be given up.

Item 2. Restriction: $\sim$(p entails $\sim A$). The restriction that p does not entail the indicative version of the consequent is not sufficient, however, to suppress all cases of the type of trivialisation referred to in the preceding paragraph and in respect to which this restriction was advanced as a necessary one. This follows from the fact that by the Principle of Contraposition an "if-then" statement, whether of the truth-functional variety or one of entailment, can be transposed into a different but equivalent statement in accordance with the formula "if p then q" = "if $\sim q$ then $\sim p$". The fact then that the consequent of a given conditional statement S is entailed by another statement R is properly described in relation to the statement which is the transposed form of S by saying that R entails the negation of the antecedent of S. It follows therefore that if all p's must be excluded from the application of the formula which are such that they entail the indicative version of the consequent, for exactly the same reasons all p's must be excluded which entail the denial of the antecedent. For in the logic of entailment it follows by the Principle of Contraposition that for any p, q, and r, "p and q entail r" is strictly equivalent to "p and $\sim r$ entail $\sim q$".[2] Let s_1, then, be any true statement; let s_2 be any statement entailed by s_1; and let s_3 be any statement whatever. Since s_1 is true, it is true that there is a p such that p is true and

[1] Let p be "All members of the club except Roger were present", let A be "Roger was present", and C, "All members of the club were present".

[2] In terms of strict implication, $pq \cdot \dashv \cdot r :=: p \sim r \cdot \dashv \cdot \sim q$. Lewis and Langford, 12·6.

p and s_3 entail s_2; and from this can be deduced by the principle of entailment just cited that there is a p such that p is true and p and $\sim s_2$ entail $\sim s_3$. The formula for a subjunctive conditional is now satisfied in a way which is not excluded by the restriction that p does not entail the indicative version of the consequent, since p in this case is s_1 and the indicative version of the consequent is $\sim s_3$. And since s_3, as stipulated above, may be any statement, in this way again an indefinitely large set of trivial subjunctive conditionals can be derived, this time to the effect that if s_2 were false, then s_3, no matter what s_3 is, would be false. The principle involved in this restriction is that which is codified, in the system of strict implication, in one of the so-called paradoxes of that kind of implication, that a self-contradictory proposition strictly implies any proposition.[1] Since by assumption s_1 entails s_2, the conjoined assertion of s_1, and $\sim s_2$ is contradictory. The restriction thus in effect requires that any p and A employed in the formula be consistent with each other, which is logically equivalent to saying that their conjoint assertion is self-consistent. It follows directly that wherever this restriction is satisfied p will never entail $\sim C$; for it could only do so if p entailed $\sim A$. Similarly it follows from the first restriction that p will never entail A. For if p and A entail C, and p entails A, then p entails C; and the restriction $\sim$ (p entails C) is violated.

Item 3. Restriction: p includes no universal conditional the consequent of which determines the universal class. When one turns from the general consideration of p's relation with the indicative versions of the antecedent and consequent, and examines the restrictions put by Chisholm upon the character of p itself in the formula of translation for statements of the form "(x) (y) if x were a ϕ and y were a ψ, then y would be a χ", one sees at once that the Principle of Contraposition can be employed to produce other restrictions which match and are equally necessary with those Chisholm has laid down. That is to say, if the restrictions laid down by Chisholm are necessary for the avoidance of trivialisation, for exactly the same reasons these additional restrictions are necessary.

Chisholm's restriction, that p contain no universal conditional without existential import, *i.e.*, one whose antecedent determines an empty class, is advanced on the basis of the necessity, in avoiding trivial entailments, of excluding from p vacuous material universal conditionals from which, as in the case of the assertion about Holbrook's election discussed above, by the use of the

[1] Lewis and Langford, 19·74, $\sim \Diamond p . \dashv . p \dashv q$.

formula trivial subjunctive conditionals can be derived. Since by the Principle of Contraposition any universal conditional in p which violates this restriction can be transposed, moving the offending portions of the conditional into the consequent of a different but equivalent one, Chisholm's restriction can be avoided and its effect nullified unless the contraposed form of the conditional is excluded also, as it is in the restriction that the consequent of no such statement determines the universal class.

Item 4. Restriction: p includes no universal conditional whose consequent includes any function not containing the variable of quantification. This is the companion to Chisholm's provision restricting the antecedents of the universal conditionals in p in exactly the same way. The purpose of Chisholm's restriction appears to be to exclude from p universal conditionals which, by the inclusion of individual constants in the antecedent, can be vacuously true and have existential import at the same time. Such a universal conditional would be the statement "(x) (Socrates is alive $\cdot$ $W(x)$. $\supset$. $M(x)$)", where "W" and "M" stand for the predicates "whale" and "mammal" respectively. By the use of this true conditional in p one can derive the subjunctive conditional that if some specific creature were a whale it would be a mammal. But because of the inclusion of the expression "Socrates is alive", which is false, in the antecedent of the above universal conditional, it is likewise true that (x) (Socrates is alive $\cdot$ $W(x)$. $\supset$. $\sim M(x)$)". Indeed the universal is true no matter what predicate is substituted for M, and, therefore, by the use of such conditionals in p one could derive subjunctive conditionals to the effect that if some specific creature were a whale it would not be a mammal, it would be able to solve differential equations, or would have any property one chooses to employ. This restriction can be avoided, too, in respect to any universal conditional which does not also contain constants in the consequent, and exactly the same trivial subjunctive conditionals derived without violating the restriction, by the transposition of the antecedent and consequent in accordance with the Principle of Contraposition. Therefore, just as universal conditionals must be excluded from p in the process of translation whose antecedents contain any function not containing the variable of quantification, so those universals must be excluded whose consequents have the same characteristic.

Item 5. Restriction: p includes no universal conditional whose antecedent includes any two functions which are logically equivalent to functions having "x is ϕ and y is ψ" and

"y is χ" as corresponding instances. This is the companion restriction to Chisholm's provision ruling out of p consequents of universal conditionals including any two functions having the above specified character. The meaning of this provision is explained and the need for ruling out universal conditions of this kind in any fool-proof formula of translation is demonstrated by Chisholm on pages 300-301. It is clear that if the effect of the provision is not to be evaded, as in the immediately preceding cases of trivialisation, by the operation of transposition, it is likewise necessary that somehow antecedents be excluded having the same forbidden character.

Whether the restrictions which Chisholm has laid down for p, taken with the above revisions and supplements, are sufficient to avoid all trivialisation of the formula of translation for subjunctive or contrary-to-fact conditionals of the form "(x) (y) if x were ϕ and y were ψ, then y would be χ" is a question still remaining to be answered; but no attempt to answer it will be made here.[1] For, important and interesting as the question is, it is far less important than a much broader question about the use of this and similar formulæ which Chisholm has raised in connexion with what he appropriately calls "accidental" universal conditionals. It seems that accidental universal conditionals, like vacuous universal conditionals, have a characteristic which makes it possible, when they have been included in p, for one to derive by the use of the formula translations which are far from equivalent to the original forms and hence very misleading. It is necessary, then, if universal formulæ of translation are to be devised, that there be restrictions upon the use of universal conditionals which will prevent such mistranslation. For when accidental universal conditionals appear in p, though they may not violate the above restrictions and may not permit trivialisation of the kind discussed above, they do permit by the use of the formula that one derive from unquestionably true statements other statements which are either highly questionable or clearly false. And that indicates that the above

[1] Since the completion of this paper Nelson Goodman's discussion of contrary-to-fact conditionals has appeared in the *Journal of Philosophy* of 27th February, 1947. Although Goodman does not deal with Chisholm's work on this problem, he shows (pp. 117-118), in treating vacuous universal conditionals and their effect upon the interpretation of contrary-to-fact conditionals, how it is possible by a simple device to construct universal conditionals which, though not vacuous, have the same effect upon these conditionals as vacuous ones. Since such universal conditionals can be constructed which do not violate the above set of restrictions, these restrictions are not sufficient for the purpose.

restrictions are not sufficient to ensure that the employment of the formula will in every case yield a correct translation.

Consider the following example different from those given by Chisholm but identical with them in principle, and artificially simplified for purposes of brevity. Suppose that it is known that a certain woman, W——, is of Rh-negative blood type, and suppose further that this woman has borne one child, after a normal pregnancy, and that this child was a normal, healthy child and was Rh-positive. It is not necessary, but it will help to bring out the special bearing of this upon contrary-to-fact conditionals, if a further supposition is made to the effect that this woman will bear no more children. Perhaps she has died, or had an accident which will prevent further childbearing, or knows that she is Rh-negative and is afraid. In any case, on these assumptions the following universal conditional is true: $(x)\ (P\ (x) \supset Q\ (x))$, where "$P$" is interpreted as the complex predicate "new born child of W——, born after a normal pregnancy, and Rh-positive", and "Q" is the predicate "healthy child, with a normal red blood count". If this universal conditional be permitted in p, then it follows that there is a p such that p is true and p and "x is P" entail "x is Q", and that hence a subjunctive conditional may be derived to the effect that (x) if x were a P, then x would be a Q. That is to say, it follows by the use of the formula that if W—— had had a second, or third, or any more children, and they had been Rh-positive and had passed through a normal pregnancy, they would have been born as healthy children with a normal red blood count. The medical evidence is that these conditional statements may very well not be true; and it is possible to estimate approximately the relative frequency with which in the long run of similar cases conditional statements like these will as a matter of fact be false.

It is necessary, then, that the employment of the formula be restricted in such a way as to prevent the illegitimate construction of subjunctive conditionals from accidental universal conditionals of this kind; and Chisholm proposes simply that all such conditionals be excluded. It is interesting to observe in the above example that if W—— decides to have no more children because she knows that she is Rh-negative and that this is a hazard to any subsequent Rh-positive children which she may have, she is by her decision rejecting the subjunctive conditional which the formula would validate, that if she had any more Rh-positive children they would proceed normally like the first. She knows in effect that although the conditional $(x)\,(P(x) \supset Q(x))$

is true it is not possible to pass validly to the conclusion that (x) (if x were a P, then x would be a Q). Now if it is necessary to restrict the use of the formula with accidental universal conditionals so that passages of this kind cannot be made, or if it is necessary to exclude such conditionals entirely, it is necessary as a means for so doing that one be able to specify what an accidental universal conditional is and how it differs from a non-accidental one. And the difficulty here, Chisholm observes, is that the only means we have of distinguishing the accidental universal conditionals is to note that, unlike the non-accidental ones, they do not warrant the inference of certain contrary-to-fact conditionals (p. 302).

No definitive decision on this matter is possible without a careful and necessarily more extended analysis of the idea of non-accidental universal conditionals than is permissible here. The terms "accidental" and "non-accidental" are of course used in a variety of ways, and not all of these involve reference to laws of the strict character which has been the ideal in the natural sciences and which was most strikingly achieved in the laws of classical mechanics. One would naturally call the occurrence of an event not accidental, for example, if the occurrence of such an event under the known conditions could be shown on accepted laws to be highly probable, though not certain. But it does seem clear, without an extended analysis, that, in the sense in which these terms are applied to universal conditionals and in which their application is relevant to the employment of those conditionals with the formula for contrary-to-fact conditionals, a non-accidental universal involves the implicit assertion in it of an indefinite set of such contrary-to-fact or subjunctive conditionals. When one affirms the universal (x) $(P(x) \supset Q(x))$ and affirms also that this universal is non-accidental in the sense in which, say, the laws of mechanics are non-accidental, what one seems to mean, is not merely that there are no P's which are also non-Q, *i.e.*, $P\overline{Q} = 0$, but also that *there can be no* $P\overline{Q}$'s. In other words, it is not merely the case that every existent P is also a Q; it is also the case that if anything else were a P, it too would be a Q. If this is so, then if (x) $(P(x) \supset Q(x))$ is true and is also non-accidental, then the subjunctive universal conditional which corresponds to it, (x) (if x were P, then x would be Q) is also true. And since if the subjunctive universal conditional is true, the indicative universal conditional is true and is also non-accidental, the assertion that any given universal conditional of the form (x) $(P(x) \supset Q(x))$ is both true and non-accidental is strictly equivalent to the assertion of the subjunctive universal

condition corresponding to it. The combined assertion "(x) $(P(x) \supset Q(x))$; and the preceding universal conditional is non-accidental" means the same as the assertion "(x) (if x were P, then x would be also Q)".

If this brief analysis is correct, it follows that in order to define a contrary-to-fact or subjunctive universal conditional one must define at the same time a non-accidental universal conditional, and *vice versa*. The problem of the definition of a non-accidental universal conditional is thus by no means a subsidiary problem among others whose solution is relevant to the problem of the definition in terms of the ordinary truth-functional logic of the contrary-to-fact or subjunctive conditional; and Chisholm has not therefore over-estimated its importance to his project of finding a formula for such definition and translation. Leaving out the problem of defining contrary-to-fact conditionals which are not universal, the solution to which, it appears, would follow readily once the problem were solved for those which are universal, the problem of defining the latter in an indicative language employing the ordinary modes of truth-functional statement construction is exactly the same problem, expressed in different terms, as that of the definition in such a language of contrary-to-fact or subjunctive conditionals. In the light of this the function of all the restrictions which have been made upon the universal conditional which may appear in p, in the employment of the formula of translation, and the question of their necessity for the avoidance of perversion of the formula, may be reviewed and reformulated. These restrictions are, one and all, means for excluding from p in the employment of the formula various forms of universal conditionals which either obviously or in more subtle ways are accidental; and their necessity in the application of the formula depends upon their function in pursuit of this end. Any of the restrictions which is necessary for this purpose is an indispensable restriction, and must be retained; any which is not so necessary may be abandoned.

At this stage in the development of the problem it must be regarded as an open question whether it is possible to define the non-accidental universal conditional in a logic in which statement-composition is effected and conditional statements are interpreted according to truth-functional formulæ in the manner of *Principia Mathematica*. If this definition cannot be effected, Chisholm concludes that the only alternative is to "accept the subjunctive as describing some kind of irreducible connection and thus reject, or alter radically, the extensional logic which most contemporary logicians have tried to apply to the philo-

sophical problems of science" (p. 302). But it may be doubted whether the project of providing exact translations of contrary-to-fact conditionals in this logic is as crucial to the acceptance of the logic and its employment in dealing with the philosophy of science as the formulation of this alternative suggests. If it should turn out to be demonstrably impossible to provide such a translation in a truth-functional logic, the demonstration that it is so should not be very surprising, nor, in the light of the abundant proof in practice of the usefulness of this logic as an instrument of logical calculation, should it be calamitous. It is not necessary, as has already been suggested in this paper, for the fruitful and legitimate use of this logic as an instrument of analysis, that it be a substitute language in which everything can be said which is said in the language which is the subject under analysis. The kind of difficulty which Chisholm has encountered in his project of translation seems properly regarded as evidence, by no means now conclusive, for the conclusion that this logic constitutes a language with special properties of its own which, although they do not prevent it from being a valuable linguistic tool, do prevent it from being a substitute or universal language into which everything in the language under investigation can be exactly translated. For those who have concluded too easily that because the logic is valuable it must constitute such a substitute language, and for those who have been disposed to use it uncritically and without reflection as if it were, the problems of translation which Chisholm has clearly presented should serve as a valuable warning.

III.—DISCUSSION.

THE UNCERTAINTY PRINCIPLE AS A PROBLEM IN PHILOSOPHY.

§ 1. Introduction.

Dr. Wisdom's review of Prof. Reichenbach's book, *The Philosophy of Quantum Mechanics*, which appeared in the January issue of Mind, is significant in that it shows clearly how comparatively small is the contribution which professional philosophers make to solving the quantum troubles of the physicists. Even when Reichenbach introduces his three-valued logic, a contribution which is considered to be of some intrinsic philosophical interest, the physicist is likely to regard it merely as a codification of nomenclature which he has for some time found useful to employ. Wisdom's review is also of interest as showing that the Uncertainty Principle, to which most physicists have got accustomed, is still full of difficulties for the professional philosopher. Now in this situation one might expect the philosopher to make contributions to the subject which, apart from any philosophical interest they may have, are also of direct use to the physicist, who is from time to time in need of constructive criticism of his fundamental concepts. The reason why this has not happened to the expected extent is not far to seek. Quantum Mechanics is a specialised and mathematical subject which few philosophers have leisure to study in detail. They are therefore naturally reluctant to go beyond the conclusions reached by the physicist in this matter.

In these lines it is desired to do three things. First, to indicate lines of approach to the Uncertainty Principle, also of possible use to physicists, which would overcome some of its intellectually unpleasant implications (§ 3); secondly, to develop a particular line of approach in some detail (§ 4); and, thirdly, to show how the whole problem may be formally connected with the problems raised by Zeno's paradoxes (§ 5). Before doing this, it is probably desirable to give a brief introduction to the formalism required for a statement of the Uncertainty Principle (§ 2).

§ 2. The Formalism of the Uncertainty Principle.

Classical mechanics uses ordinary continuously variable quantities $a, b, \ldots$ obeying ordinary algebra. In quantum theory the assumption that all physical variables can vary continuously is dropped and observable quantities are specified by 'operators',[1] A, B, ...

[1] This term is used instead of the more correct one 'hypermaximal hermitian operators'.

Each operator contains within itself a certain amount of information; in particular, whether or not the observable quantity which it specifies is capable of continuous variation; and, if not, one can also obtain from it the discrete set of values which the observable quantity can in fact assume. This discrete set, or continuous range, of values associated with an operator are called its 'proper values'. Now just as in ordinary algebra a variable has no reciprocal when it is zero, so an operator has no reciprocal if one or more of its proper values vanish. Thus the ordinary algebra and the algebra of operators differ in that it is not in general possible to assume that an operator has a reciprocal.

More important from the present point of view is another difference which arises as follows. Most operators can be represented by a square array of numbers ('matrices'). If in such an array the only non-zero elements are in the main diagonal (*i.e.*, the one which starts at the left-hand top corner), the operator is said to be diagonal. Its diagonal elements are just its proper values. Now any *one* operator may by a suitable transformation be thrown into diagonal form. For such diagonal operators A, B, . . . just as for symbols $a, b, \ldots$ of ordinary algebra, the commutative law of multiplication [1] $i(AB—BA) \equiv [A, B]$ (say) $= 0$, $ab - ba = 0$, holds. The important point is that the necessary and sufficient condition for the possibility of throwing *two* operators, A, B *simultaneously* into diagonal form is $[A, B] = 0$. Thus the commutative law of multiplication does not necessarily hold for operators.

It is now possible to state the Uncertainty Principle in general terms. Suppose two quantities a, b of a certain physical system, and represented by operators A, B respectively, are measured simultaneously with uncertainties δa, δb respectively. Suppose further that the observable quantity represented by [A, B]—which incidentally has no classical analogue—has the value $\overline{[A, B]}$ in the same system. Finally let $|x|$ denote the quantity $+x$ or $-x$, whichever is non-negative. Then the Uncertainty Principle states:

$$\delta a \cdot \delta b \geqslant \tfrac{1}{2} |\overline{[A, B]}| \quad . \quad . \quad . \quad (1)$$

Thus δa and δb can in general both be zero only if A and B commute. Relations of type (1) exist for any pair of observable quantities, and they are called 'uncertainty relations' if the right-hand side is non-zero. In ordinary algebra, and therefore in classical physics, the symbols representing observable quantities commute always so that (1) becomes $\delta a \cdot \delta b \geqslant 0$; there are evidently no uncertainty relations in classical physics. Suppose the relation $\delta a \cdot \delta b > 0$ holds in a certain case. The assumption $\delta a = 0$ would then clearly make the whole problem unintelligible, since it entails $\delta b = \infty$, so that nothing at all can be known about the quantity b.

[1] The introduction of $i \equiv \sqrt{-1}$ is not essential; it merely ensures that the operator [A, B], as all other operators which occur in this paper, is hermitian.

In conclusion it should be mentioned that there are two operators which always commute with all others : the zero operator O and the identity operator I. They are defined by the condition that all their proper values are zero and one respectively.

§ 3. The Fundamental Philosophical Problem.

Operators which mutually commute, and therefore do not give rise to uncertainty relations, are (X_1, X_2, X_3), representing the space co-ordinates of a particle ; (P_1, P_2, P_3), representing the components of the linear momentum of a particle ; and certain components of the electric and magnetic fields in vacuum. Examples of operators which do not commute are the pairs X_r, P_r $(r = 1, 2, 3)$, the energy and the time, etc. In order to obtain these results from the mathematical theory use must be made of analogies with problems which are already well understood. Alternatively, relations of type (1) can also be obtained from the careful consideration of theoretical experiments. The former method has the drawback that it gives very little real insight into the physical processes involved, while the latter is often somewhat tedious.

Now each extension of quantum theory into a new field requires the setting up of commutation relations, and in the first instance the question is raised : Which operators commute—which do not commute ? Thus there arises a problem of fundamental importance alike to the philosopher and the physicist : What is the intrinsic difference between observables whose operators commute and those whose operators do not commute ? Since the physicist can find the answer to the commutation puzzle in all particular cases by the special devices mentioned above, he rarely raises the problem in this fundamental form, but, instead, presses on with his investigations. It does arise for the philosopher since a true understanding of the quantum formalism depends on an answer to it. From what has been said it is evident that if the philosopher can find an answer to this fundamental problem, it is likely to be of considerable assistance to the physicist.

It may be useful to deal with one possible answer straight away. One might say that two operators commute if, and only if, the measurements of the observables they represent do not interfere with each other in any (theoretical) experiment, *i.e.*, if the possibility $\delta a = \delta b = 0$ exists in the sense of equation (1). This answer naturally begs the question, since it is merely a special way of *stating* equation (1) instead of *explaining* it. The fundamental problem why the measurements of certain observables interfere (in the sense $\delta a \cdot \delta b > 0$), while others do not interfere, remains quite untouched.

A more hopeful line of argument is the following. The measurement of any observable is preceded by the definition of that observable, and it is therefore possible that the interference of two experiments designed to measure two distinct observables of the same

physical system, expresses merely the fact that the original definitions of the physical quantities concerned contained an element of incompatibility. With this approach the fundamental problem takes a slightly different form: What is the element of incompatibility which has intruded into the definitions of physical quantities such that some of them cannot be measured simultaneously with unlimited accuracy?

§ 4. Discussion of a Certain Principle.

In this paragraph it is proposed to illustrate the *type* of answer which may conceivably bring about a solution of the fundamental problem. The particular answer to be put forward is contained in the statement of a general principle which is now to be elucidated.

Consider a quantity $a(t)$ which depends on a parameter t such that the differential coefficient $\dot{a} \equiv da/dt$ exists within the interval considered. If the dependence of a on t is given explicitly (*e.g.*, $a = 5t^2$) so that $\dot{a}$ can be calculated ($= 15t^2$), and if both a and t represent observable quantities, it is clear that, provided t can be measured accurately, a and $\dot{a}$ can both be known accurately for any value of t: one need merely measure t and substitute in a and $\dot{a}$. If, however, the dependence of a on t is *not* known explicitly, it has been the practice since Newton's time to assume, precisely as in the first case, that a and $\dot{a}$ can both be known accurately for any value of t. The present point of view will be that the two cases are physically not fully equivalent.

The principle takes the following form: 'If a depends on t in an unknown manner such that $\dot{a} \neq 0$ within a non-zero interval $t_2 - t_1$, then the values of a and $\dot{a}$ (or t and $\dot{a}$) corresponding to $t = t_0$, ($t_2 > t_0 > t_1$) cannot both be known accurately. In all other cases they can be known accurately.' An arrangement with zero experimental error is of course contemplated. An explanation of the principle follows.

Having defined a physical observable $a(t)$, in which a depends on t in an unknown manner, by the relation

$$[\dot{a}(t)]_{t=t_0} = \lim_{t_1,\, t_2 \to t_0} \frac{a(t_2) - a(t_1)}{t_2 - t_1}, \quad . \quad . \quad . \quad (2)$$

it becomes necessary to consider methods of measuring it. The appearance of (2) as the slope of the tangent at $t = t_0$ to the curve of a against t, is here clearly irrelevant, since no such curve can be drawn, the explicit mathematical dependence of a on t being unknown. It may still be argued that one might measure $a(t)$ accurately, and a large number of times, at accurately known values of t, draw a smooth curve, and find the tangent at $t = t_0$. This procedure is, however, inadmissible, since one is interested in accurate values of $\dot{a}$ and not in approximations; and evidently all portions of the graph which are not experimental points *are* approximations. An idealised

apparatus is of course assumed here : it allows experimental results to be represented by geometrical points.

One can readily show that it is impossible to overcome this difficulty. For the existence of $[\dot{a}(t)]_{t=t_0}$ implies that the curve of a versus t is continuous in some non-vanishing neighbourhood of $t = t_0$, however small, and so contains a non-enumerable number of points in that neighbourhood. But the number of experiments that can be carried out, or even conceived, is enumerable. One cannot therefore construct *accurately* the neighbourhood of $a(t_0)$, however small, by experiment. The exact tangent to the curve can therefore not be obtained experimentally.

Since a passage to the limit cannot occur in any experiment, being an essentially mathematical notion, it is evident that for physical applications (2) must be replaced by

$$[\dot{a}(t)]_{t=t_0} = \frac{a(t_2) - a(t_1)}{t_2 - t_1} \quad (t_2 > t_0 > t_1,\ t_2 - t_1 \equiv z) \qquad (3)$$

where z is small but non-zero. It now follows that if $\dot{a}$ is known accurately, the value of a belonging to the same value t cannot be known accurately. For, as is evident from the right-hand side of (3), a measurement of $\dot{a}$ involves a non-zero parameter interval z so that there is some ambiguity as to precisely which sharply defined value of a corresponds to it : any of the values of a lying between $a(t_2)$ and $a(t_1)$ has an equal right to be considered as a " simultaneous " measurement of $a(t_0)$.

It would appear from (3) that $\dot{a}$ can never be measured accurately, since the interval z is not properly defined. However, the interlocking in the definitions of physical quantities may make it possible to obtain an accurate value of $\dot{a}$ from accurate measurements of quantities other than a, $\dot{a}$ and t. The question arises whether a measurement of $\dot{a}$, conceived as accurate, derived in *this* way is compatible with an accurate observation of the corresponding a. In order to answer this question one merely needs to consider the case where $\dot{a}$ has a value which is not zero. This means that a must have changed during the experiment, which therefore again involves a non-zero parameter interval, so that the previous arguments apply. For similar reasons it follows that if either a or t is measured accurately the corresponding value of $\dot{a}$ is uncertain. One therefore finally arrives at the stated formulation of the principle.

If attention be confined to a and $\dot{a}$, it is possible to put this conclusion into the following form. Let T, A, $\dot{\text{A}}$ be the operators which represent (in the sense of § 2) variables t, a and $\dot{a}$ respectively. Then our principle states : ' If $\dot{\text{A}} \neq 0$, $[\text{A}, \dot{\text{A}}] \neq 0$, $[\text{T}, \dot{\text{A}}] \neq 0$; in all other cases operators commute '.[1] It follows that

[1] It may be objected that with $\dot{\text{A}} \neq 0$ it is possible to have a relation $[\text{A}, \dot{\text{A}}] = 0$ or $[\text{T}, \dot{\text{A}}] = 0$, namely when A (or T) is the identity or the null operator. The nature of A and T is, however, such that both A and T must have a continuous range of proper values so that these two extreme possibilities do not in fact arise.

$$[A, \dot{A}] = 0 \text{ entails } \dot{A} = 0 \quad . \quad . \quad . \quad (4)$$

Using (1) and (4) one obtains

$$\delta a \cdot \delta \dot{a} = 0 \text{ entails } \dot{a} = 0 \quad . \quad . \quad . \quad (5)$$

Indeed, recalling our comment on equation (1),

$$|\delta \dot{a}| < \infty, \ \delta a = 0 \text{ entail } \dot{a} = 0 \quad . \quad . \quad . \quad (6)$$

The above represents the particular example with which it was desired to illustrate the type of contribution philosophers might make to the uncertainty problem of the physicist. That the argument offered is in fact the correct answer to the fundamental problem of § 3 can at most be regarded as a theoretical possibility. For even if this particular argument be acceptable to philosophers—and this may well be too much to hope for—its final fate must depend on the validity of (4) in quantum mechanics. It is intended to turn to this problem elsewhere, since it belongs to physics rather than to philosophy. Here it may merely be noted in passing that, at least in the simplest case, a component P_j of the linear momentum is given by $P_j = m\dot{X}_j$, m being the mass of the particle and the differentiation being with respect to the time. Thus (4) is certainly in agreement with the relations

$$[X_r, X_s] = [P_r, P_s] = [X_j, P_k] = 0,$$
$$m[X_j, \dot{X}_j] = [X_j, P_j] \neq 0,$$
$$(r, s, j, k = 1, 2, 3 ; \ j \neq k),$$

which were mentioned in § 3.

§ 5. The Relation with Zeno's Paradoxes.[1]

It may be supposed that, since the validity of (4) depends largely on its success in physics, further discussion of that relation can do little to clarify the issues involved. Nevertheless, it may be useful, for the sake of argument, to *assume* the truth of (4). Conclusions derived with the aid of this assumption may shed further light on the problem and may well affect our confidence—or lack thereof—in the principle stated in § 4.

Consider Zeno's paradox of the arrow. Attention is fixed to this paradox because, so it appears to the writer, it presents the problem of motion in the most undiluted form. It will be recalled that Zeno's argument was: 'Suppose an arrow is flying from a point X to another point Y. At each instant during the flight it may be supposed to be at rest at a certain point. Since the period of the flight is made up of instants it is therefore permanently at rest, in contradiction with hypothesis.'

Let the problem be further simplified by assuming that the arrow describes a straight line (not a curved trajectory), and that the

[1] The writer is greatly indebted to D. L. Levi, M.A., for some stimulating discussions of the subject-matter of this paragraph.

co-ordinates be chosen in such a way that the direction of the flight is parallel to the x-axis. Attention is concentrated on a fixed point P of the arrow. At X let the x-co-ordinate of P be zero, at Y let it be l. x is a function of the time (not known explicitly), and $\dot{x}$ is the velocity of the arrow, the differentiation being with respect to the time. The first sentence in the paradox asserts $\dot{x} \neq 0$. The second sentence implies $\delta x = 0$. The conclusion is $\dot{x} = 0$.

If equation (4) is assumed, its special form (equation (6)) shows that $\dot{x} = 0$ does in fact follow from $\delta x = 0$. The whole problem resolves itself: the assumption $\delta x = 0$, made by Zeno, is inconsistent with his initial assumption $\dot{x} \neq 0$. This is broadly equivalent to Bergson's assertion that the arrow never *is* at a point of its path.[1]

Since the uncertainty relations between position and momentum are widely known, the connexion with Zeno's paradox has of course been generally noticed.[2] By itself this correlation is, however, of little value, since the paradox can be applied to *any* two observable quantities, such that one is a continuous function of the other. If the argument of § 4 be sound, it can be used to refute the paradox in just this general form.

P. T. LANDSBERG.

[1] H. Bergson, "Creative Evolution," first ed., Macmillan, 1928, p. 325.

[2] See, for instance, A. Ushenko, MIND, April, 1946. See also L. de Broglie, *Matter and Light*, tr. A. Johnston, Allen and Unwin, 1939, p. 254.

IV.—CRITICAL NOTICES.

Problems of Men. By JOHN DEWEY. New York: Philosophical Library, 1946. Pp. 424. $5.00.

THE thirty-one papers which comprise Dr. Dewey's latest volume are all reprinted from periodicals: only the Introduction appears for the first time. The longest of the papers ('Logical Conditions of a Scientific Treatment of Morality') was previously printed fifty years ago, and provides a Kantian 'Prolegomena' to his own later critiques of reason and moral judgment. All the other papers belong to the years 1935-45. They are arranged as follows:—

Part I.—'Democracy and Education'—ten papers.
Part II.—'Human Nature and Scholarship'—seven papers.
Part III.—'Value and Thought'—ten papers.
Part IV.—'About Thinkers'—four papers.

The papers in the first two parts are almost entirely concerned with the making of educational policy—the 'Need for Orientation', as Dewey phrases it. They expound and defend his well-known views on the teaching of scientific methods and attitudes, and on the essential connexion between these and the democratic way of life. The different papers were originally addressed to widely differing audiences—teachers, education officers, college presidents and Rotarians, and the group of wealthy college alumni who are the readers of the super-magazine *Fortune*. There is inevitably a very great deal of repetition in these various addresses, and it is difficult to see why they have *all* been brought together for republication. The best are those which deal with the more specific and definite questions; for example, the overcrowding of the high school time-table, the place of languages in a college curriculum, the relations of the local education officer to the teachers in the local school, the proper methods of teaching social studies. A narrower selection should certainly not omit Dewey's *Fortune* article of 1944—a smashing reply to President Hutchins and his plan for an education based in part on the scheme of a mediæval university. Dr. Dewey is particularly incensed against Mr. Hutchins's 'theological fellow-travellers' who cling to the notion of a 'natural' man and a 'spiritual' man; but even these rouse him less than the so-called liberals in the camp, who harbour the dangerous notion that education might be founded upon languages and literature rather than upon science. Dr. Dewey goes so far as to say:—

> "The attempt to re-establish linguistic skills and materials as the centre of education, and to do it under the guise of 'education for freedom' or a 'liberal' education, is directly opposed to all that democratic countries cherish as freedom" (p. 149).

Dewey's influence in his own country has been immense—both with the people at the top (or the people who have come to the top) and with the humblest neighbourhood school-teacher. Many of the papers of this last decade are defensive, but what defence ! Entirely unshaken in his own faith, and ready to learn from men's triumphs and disasters of fifty years and more, he still writes with the greatest power and penetration and generosity :—

> " The oldest hath borne most : we that are young
> Shall never see so much nor live so long."

The subject-matter of the second two parts is of very different interest. In Part III we have first the long paper of Dewey's Chicago days, to which I have referred,[1] and then a series of nine papers contributed to the *Journal of Philosophy* from 1941 to 1944; in Part IV we have biographical-critical sketches of William James and James Marsh, and a short discussion of Whitehead's philosophy. All these papers are philosophical in character, and many of them could hardly be understood at all by anyone but a student of contemporary philosophy. I shall give the remainder of my space to a consideration of some of these papers.

Of the early paper, Dr. Dewey says that it provides an " anticipation of the direction in which I have moved during the intervening fifty years ". It is, in fact, a very good introduction to his doctrine of the " continuity " of ethical and other judgments. He was already less concerned with science as a system of general laws, than with those final judgments which mark the conclusion of an inquiry. In such a judgment, use is made both of what we " apprehend " in the way of particular facts, and of the general principles of a science or sciences. He gives as an example, " locating the source of an epidemic of typhoid fever " (p. 218). Such judgments are ' practical '—they have an aim which involves the attitudes and characteristics of the judger ; but these are ' standard ' attitudes which do not differ materially from one inquiry of the sort to another —fundamentally the attitude of the scientific inquirer is the same whatever his field may happen to be. An ethical judgment, on the other hand, is one in which particular attitudes of the judging subject cannot be ' taken for granted ' because they do vitally affect the final judgment made. Such attitudes are always directed towards some *object*, and the nature, character, causal properties of this object are therefore relevant to the ethical judgment. It follows, therefore, that the general laws of any of the sciences *may* happen

[1] This is evidently the paper to which reference is made (under a slightly different title) on page 33 of *The Philosophy of John Dewey*. The suggestion is there made that it was published at the same time as the *Studies in Logical Theory, i.e.*, in 1903. The bibliography lists the title (as given in the present volume) under the year 1903, but states that it was then reprinted from the First Series of the Chicago University Decennial Publications. No earlier reference is made, however, in the bibliography.

to be relevant to a particular ethical judgment. But Dewey was concerned to show that in every case the laws of psychology and sociology were bound to be relevant. For by knowledge of ourselves and of our society we should learn to make ethical judgments which would in fact meet our most permanent and important desires. And just as Dewey believed that the pursuit of the same inquiries by the same methods would, in the long run, lead to the establishment of common agreed conclusions, so he found it not incredible that the more enlightened we became about ourselves and our society, the nearer we should all come to agreement as to aims and valuations.

The basic notion of the 'continuity' of ethical and other judgments is called by Dewey, in Kantian fashion, 'the postulate of continuity of experience'. It served to protect "the integrity of the moral judgment, revealing its supremacy and the corresponding instrumental or auxiliary character of the intellectual judgment", and at the same time protected "the moral judgment from isolation (*i.e.*, from transcendentalism), bringing it into working relations of reciprocal assistance with all judgments about the subject-matter of experience, even those of the most markedly mechanical and physiological sort" (249). The language is the language of Königsberg, but the *integrity*, *supremacy* and *reciprocal assistance* had certainly acquired a new and startling meaning. Dewey does not in this place mention another very important consequence of this principle: that he has established the same sort of test for intellectual and moral judgments. It is notoriously difficult to hold a simple, analytical correspondence theory of the truth of value propositions. Dewey's view of moral judgments is that they are true if they 'work'. And unless a similar sort of theory can be made to work for intellectual judgments, we shall find that after all the 'continuity' is broken—there are, after all, two worlds, a world of value and a world of fact. It is against this above all that Dewey has set himself. So that the 'continuity' doctrine has (I feel sure) been a powerful influence in his mind in favour of a pragmatic theory of truth and away from an analytical correspondence theory.

The recent papers from *The Journal of Philosophy* may be divided into the more constructive (Numbers 6, 7 and 8), and the more purely controversial (Numbers 2-5, 9 and 10). Paper 6 ('By Nature and by Art') discusses the revolution of scientific methods from empirical to experimental, and expounds Dr. Dewey's views on the correlatives 'Theory and Practice', 'Objective and Subjective', 'Individual and Social', "Science and Art'. Number 7 ('How is Mind to be known?') explains the 'behavioral' view of the data of introspection. Number 8 ('The Objectivism-Subjectivism of Modern Philosophy') discusses the different meanings that this dilemma has had at various periods in the history of philosophy. Dr. Dewey suggests, for instance, that the nearest thing in Greek philosophy was the dilemma 'by nature' or 'by law, convention, institution'.

For contemporary philosophy, the issue is the relation of the objects of direct experience to the causal conditions of these objects which form the subject-matter of science.

It is naturally impossible to summarise briefly the diverse contents of the six controversial papers; but it may perhaps be of some help to indicate three important lines of criticism against Dewey's philosophy, which he is here concerned to answer.

(1) That Dewey's philosophy fails to take proper account of elements of immediacy to be found in judgments of value and in certain simple empirical judgments. In the case of value-judgments the immediately-given element is a feeling of enjoyment or satisfaction which accompanies the experience of what is of value: in the case of the empirical judgments, it is a perceptual element. Two papers contributed by Mr. Philip Rice to the *Journal* deal with value-judgments, and Dewey's replies are here reproduced as papers 2 and 3 of this part: papers 4 and 5 supplement the arguments on the same and related questions. It seems to me that the objection could have been put much more simply and strongly by someone who held the view that value-judgments are not wholly descriptive, but that they are used as *expressions* of these 'feelings'. Mr. Rice seems to take a half-way house, and to hold that these feelings are relevant evidence and yet are 'subjective' in some sense in which percepts are not. Dr. Dewey has no difficulty in exposing this doctrine of subjectivity: "if the fact that certain occurrences centre in a particular organism justifies the conclusion that the event thus conditioned is private and 'subjective', the doctrine that colours and overt movements *as perceived* are also private seems logically to follow" (p. 264). The immediate element in judgments of perception is one of the topics raised by Lord Russell's *Inquiry into Meaning and Truth*, and will be discussed later (paper number 10). It is interesting to note, however, that the two alleged 'immediately given elements' are connected by Dewey in his review of Russell's *Religion and Science* which is to be found as paper number 4 of the second part of this volume.

(2) That Dewey does not deal with judgments in a strictly *logical* fashion; that is to say, he does not really tackle the question of truth and falsity, or validity, but confuses this question with psychological questions about how judgments come to be made, and about their causal consequences. Here, Russell is the chief protagonist, and Russell's arguments about validity naturally bring in also the question about the *given* in perception. For he holds that the truth of more elaborate empirical judgments must be determined, at least in part, by the truth of simpler judgments about particular matters of fact, and that the test of the truth of these simpler judgments involves reference to an element immediately given in perception. So that (I think Russell would say) it is partly *because* Dewey fails to give a clear account of particular perceptions, that he is unable to give a clear account of the tests of the truth of more

complicated judgments—and must go off into irrelevancies about their psychological and sociological consequences.

(3) That Dewey's philosophy confuses the state of mind of the inquirer with the state of the objects into which inquiry is being made. Mr. D. S. Mackay asks, in an article in the *Journal of Philosophy*, " What does Mr. Dewey mean by an ' Indeterminate Situation ' ? " Dewey's reply (paper number 9) is supplemented by Part IV of his reply to Lord Russell, who had asked the same question in the *Inquiry into Meaning and Truth*. In what sense does Dewey mean that a doubtful situation is itself doubtful ? A parallel question has to be asked about the final end of inquiry : In what sense does Dewey mean that this final situation is a modification of the original situation which provoked the inquiry ? Mr. Dewey's answers to these questions are very hard to follow. Man is a part of nature, not an external spectator : what he knows is always an environment, or that part of nature-outside-the-organism which is reacting with that part of nature which constitutes the organism. Hence my doubt (before inquiry) and the settlement of that doubt (at the end of the inquiry) are changes that take place in nature. (See p. 351.) That may be ; and if it is so, no doubt we *could* make these changes the object of further inquiries : but have these changes any part in that ' situation ' which actually provoked the original inquiry ? But I do not know whether I have understood what Mr. Dewey means : for he has also said elsewhere that he is not guilty of the ' Irish bull ' which would allow an inquiry to change *the object of knowledge :* that all he means is that the *object as known* changes as the inquiry proceeds (*The Philosophy of John Dewey*, p. 547).

English readers will have a particular interest in paper 10, ' Propositions, Warranted Assertibility, and Truth '. For this paper has a place in the notable controversy which began in 1908, with Russell's ' Transatlantic Truth '. The following mark the later phases :—

1938 : Dewey's *Logic, The Theory of Inquiry*, Chapter VIII.
1939 : *The Philosophy of John Dewey*, Russell's comments on the *Logic* and Dewey's reply.
1940 : Russell's *Inquiry into Meaning and Truth*, Chapter XXIII.
1941 : Paper 10, published in the *Journal of Philosophy*.
1947 : Russell's *History of Western Philosophy*, Chapter XXX.

The controversy deals with the nature of truth and knowledge and the present paper is a contribution of some importance, if only because it serves to make clearer which differences arise out of differences of vocabulary and which are deeper.

As both acknowledge, Dewey and Russell use nearly all the keywords differently, including ' knowledge ', ' truth ', ' proposition ' and ' experience '. In the case of knowledge, the example given in the early paper is as a matter of fact an example of the simplest judgment which he is willing to call ' knowledge '. Imagine a

scientist looking through a microscope at a smudge on a glass plate. What may he be *knowing ?* " Redness-here-now " is Russell's first and fundamental answer. This, Dewey refuses to call ' knowledge ' at all, though he would agree to call it an ' apprehension '. Does the scientist ' know ' the general principle that ' smudges of this kind are always associated with typhoid fever ' ? This, according to Dewey's terminology, is not knowledge, either: it is a proposition which may be *affirmed* but not *asserted.* For Dewey, only the final judgment which makes use both of what we apprehend and of what we affirm, is to be called ' knowledge ' : so that what a scientist may know, is ' what caused this epidemic of typhoid '. Dewey therefore declares that the so-called ' immediate knowledge ' is not knowledge, and even that the Greeks had little, if any, knowledge about the physical world (p. 312).

I think it is mistaken to say that Dewey does not attempt to answer the question : What makes a true proposition true ? He is concerned, not merely with the removal of the causes of doubt, but with the removal of the *need* of doubt. (See Russell in *The Philosophy of John Dewey,* p. 147.) He is concerned with what *warrants* a believing, what justifies an assertion as true (p. 347). But with such a starting point (his notion of knowledge), he must find it very difficult to provide an answer. He cannot, as Russell would, try to explain the truth of what is known, by reference to the truth of simpler propositions : for ' truth ', like ' knowledge ', is reserved for the complicated prototypes upon which his theory is based—" truth and falsity are properties only of that subject-matter which is the *end,* the close of the inquiry " (p. 340). His only attempt at analysis is to define knowledge as ' warranted assertibility ' (p. 332 and the *Logic,* p. 9). The full meaning of this phrase can be gathered only from a wide study of his writings : it is only a rough and ready approximation to say that a warranted assertibility is one which brings, as a causal consequence, the resolution of the particular doubt which gave rise to the inquiry. Dr. Dewey fights hard against the wider pragmatic notion that a true belief is distinguished from a false by the *general* consequences of holding it, including, for example, the satisfaction that a person might gain on account of special desires. It seems to me that Russell's criticisms on this point in the *Inquiry,* repeated without any abatement in the *History of Western Philosophy,* are neither helpful nor altogether just. What Dr. Dewey wants is to limit his test to those particular effects which have a psychological connexion with the particular doubting or puzzlement—in the case of the example, to that belief about the causes of the epidemic which gives a stable, useful, satisfying view of the question.

I think it is difficult to say how, on his definition of knowledge, we are to distinguish the two questions : " Was the belief true ? " and " Was the belief known to be true ? " It is difficult also for him to explain the procedure of improving upon " warranted assert-

ibilities". He holds that all beliefs are liable to revision: but what does this mean if we abandon (as Dewey does) all notion of an absolute truth? Surely his revisionist empiricism requires the notion of truth as an ideal upper limit, just as the notion of the fallibility of all actual measurement presupposes some ideal of correct measurement.

I come now to a more fundamental difficulty. I think Dr. Dewey is quite mistaken in supposing that we could possibly define the truth of a belief by reference to the consequences of such a belief—or its causes either. How do we recognise a true statement? I ask a little boy what is the colour of this sheet of paper, or how much is seven nines. And if he says 'white' and '63', I know that he has given the true answers, no matter what events in his past history led him to answer as he did, and no matter what are the consequences for him of giving these answers. I know his answer is right because I know the *rule* for identifying objects as 'white', and the *rule* for multiplying seven by nine. I check his answers by applying the rules which he ought to apply: I make an *adjudication* under the rule and this will be understood and accepted by everybody else who knows the rule: those who do not know the rule can easily be taught it.

No doubt this would sound dogmatic to Dr. Dewey: for I know no way of checking his answer except by an operation of my own. But if we are to take Dewey seriously as attempting to answer the question of validity, his own answer (by reference to the consequences of the boy's belief) will only *postpone* the crucial question. For we should still need some *rule* for identifying those special consequences which make the belief true. We should still need to learn "the function of consequences as necessary tests of the validity of propositions" (*Logic*, preface). Such a rule must be learned and applied, and its application would have to be just as 'dogmatic' as I was in applying the rule for seven nines.

So that by referring to the consequences, Dr. Dewey would not be able to answer the question of validity in a purely *descriptive* way. And what he seems to want to do is to make his answers factual, causal, descriptive. This he might seem to do if he could give us, not a general rule for identifying the consequences he has in mind, but a description of the consequences of *each* true belief, one by one. But even then the description will be general (and so will have to be applied); and obviously it is part of what we mean by a 'procedure', such as the determination of truth or falsity, that it should be applicable to *new cases*.

Curiously enough, Dr. Dewey points out some of these difficulties when he is criticising certain passages from Russell's *Inquiry into Meaning and Truth*. For Russell there says that the question, with regard to the simplest beliefs, as to whether they are or are not cases of true belief, is to be answered by reference to their causes (pp. 333-334 and 342-343. The quotation is from page 160 of the English

edition of Russell's book). The judgment "That is red" (Russell says) must be true if it is caused by what it asserts, *i.e.* by the presence in the assertor's mind of a percept of redness. In his comments, Dewey writes as though Russell were giving a causal account of knowledge as well as of truth.[1] He says that our knowledge of the *causes* of our judgments is highly theoretical and dubious: so that if Russell were right in his analysis, no such judgment could be immediately known to be true. Moreover Dr. Dewey explicitly says that at best Russell's theory never "gets beyond specifying the kind of case *in general* in which the relation between an event, as causal antecedent, and a proposition, as effect, is such as to confer upon instances of the latter the property of being true" (p. 342). In other words, we should still have a general rule under which we should have to adjudicate. And in my view, precisely the same comment can be made upon Dewey's own attempt to specify certain consequences as conferring truth upon a belief. And neither the causes-rule nor the consequences-rule is in fact the right rule. For I can *know* that the boy gives the right answers without inquiry into either causes or consequences.

It will be seen that Dr. Dewey tries to make the theory of knowledge into a factual description of *processes* of coming to know. He avoids recognising the notion of a *procedure* as something which, unlike a process, may be said to have been correctly or incorrectly carried out. His own theory of knowledge is opposed to 'the epistemological theory', according to which the conditions of validity in a given inquiry are "of an entirely different order from the facts of actual investigation" (p. 294). And he confesses that he is "suspicious of the whole epistemological industry" (p. 344). One can see that there underlies this suspicion a perfectly justifiable conviction that different inquiries do, in certain respects, follow different procedures, so that the meaning to be given to 'well-established' or to 'explanation', in history or the social sciences, cannot be pre-decided by a consideration of mathematics only, or of mathematical physics only, or of simple judgments of perception only. He is also aware that canons of validity may *change*. But I think, none the less, we are obliged to admit that questions of the validity of statements or inferences, like questions of validity in general, are indeed of an entirely different order from questions of fact. They can be settled only by those who are able to apply the relevant rules, and settled only *for* those who understand those rules.

Many other important issues are raised in the controversial papers;

[1] Russell himself, in his chapter on Dewey in the *History*, writes as follows on the subject of truth :—

> "The main difference between Dr. Dewey and me is that he judges a belief by its effects, whereas I judge it by its causes where a past occurrence is concerned. I consider such a belief 'true', or as nearly 'true' as we can make it, if it has a certain kind of relation (sometimes very complicated) to its causes" (p. 853).

and on many of them Dr. Dewey seems to me to say things that are true and important and likely to be overlooked by the analytical schools. For example, he is not content to view a science merely as a system of general laws: he wants to follow out our application or use of these laws. And only by doing so, I think, can we distinguish clearly between physics and the mathematical theory of physics, between arithmetic (or logic) and an uninterpreted calculus.

In Part IV of *Problems of Men* we enter a milder, autumnal climate. The first of these four studies of philosophers is devoted to James Marsh of Andover Seminary, who was an early American Transcendentalist, a disciple of Coleridge and of Kant. He believed, as Coleridge did, that the Christian view of life was inherently alien to the philosophy of Newton and Locke, and that the new German Idealism would bring in a climate of opinion in which Christianity would find itself at home for the first time. This would not happen, however, without great changes in Christian thought, a purging of materialism and trivial externalism both in doctrine and in life. And in his modest way, he appears to have thought it his task to prepare America for the great change, to help to introduce America to the philosophy of the 'modern world'. Dewey's treatment of his subject is sympathetic and generous, and one can see that he must regard his own life-work, both as a teacher and as a thinker, in somewhat the same light.

The first of the two papers on William James consists of comments on Prof. R. B. Perry's two volumes on *The Thought and Character of William James* published in 1935. The comments naturally suggest many reflexions and, in particular, Dewey stresses the importance of James's childhood, and of his varied career through painting and medicine to psychology and philosophy. The second of the papers shows that, even when he was writing the *Principles of Psychology*, James was already entertaining his radical hypothesis *that consciousness does not exist.* (See the autobiographical sketch contributed by Dewey to *Contemporary American Philosophy*, 1930.)

The last paper in this book is on Whitehead's philosophy. Dr. Dewey asks whether that philosophy is to be interpreted primarily in an intuitive, Rationalist sense, as a system of necessary truths; or primarily in an instrumental sense, as a set of wide experimental generalisations whose origin and aim is inquiry. Dewey naturally prefers the second interpretation, and he asks: In which direction is Whitehead's influence (certain, in his view, to be great and lasting) more likely to affect philosophical thought? It will be seen that this raises the further question: What will be the weight of the influence of Dewey's own writings on philosophical inquiries? That it will be great and lasting, there seems no reasonable doubt.

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L'Estetica dell' Empirismo Inglese. By MARIO M. ROSSI. Florence: G. C. Sansoni, 1944. 2 vols. Pp. 498 + 471. L.300.00.

PERHAPS the best way to describe this work would be to say that it performs the same service for æsthetics as Selby-Bigge performed for ethics when he published *British Moralists.* After a long introductory essay, Signor Rossi presents the reader with extracts from the writings of nineteen British authors, from Bacon at the beginning of the seventeenth century to Jeffrey at the beginning of the nineteenth. The text of the selections consists, for the most part, of translations specially made by the compiler. Each is preceded by a short commentary on the life and writings of the author. Copious explanatory and critical footnotes have been added to enable readers to understand the exact significance of the texts.

To his eclectic labours Signor Rossi has brought not only great industry, wide learning and much critical acumen, but also a knowledge of British literature, history and manners that far exceeds what could be reasonably expected of an Italian editor. A foreigner who can correct Saintsbury on a point of literary criticism (p. 177), query Doctor Johnson's opinion of an obscure poet like Blackmore (p. 733), and distinguish a *tuath* from a *sept* (p. 311), is surely a rare and valuable curiosity. As to the accuracy of the translations we entertain no serious doubts, although circumstances have prevented us comparing them with the originals. Our one real regret is that Signor Rossi, no doubt for urgent reasons, has made his renderings of Dugald Stewart from a French translation, and not from the English original.

The first thirty-eight pages of the *Introduzione,* though learned and acute, are not likely to interest any but specialists. The historical sketch that follows is of much greater interest, and in presenting my account of the contents of the book—an account which differs on some points from Signor Rossi's—I shall make a liberal use of it. British æsthetic theory between Bacon and the Utilitarians passed through four distinguishable stages, a literary period that began in Elizabethan times, a psychological period inaugurated by Locke's *Essay,* a technical period dating from the foundation of Reynolds' academy, and an inductive period coinciding in its origins with the publication of Reid's *Essays.* The landmarks may be a little artificial, but they represent real changes in the manners of society and their literary expression.

(A) *The Literary Period* (1600 +).

Bacon distinguished three fundamental factors in the human understanding, reason, memory and fantasy, and three corresponding forms of cultural activity, philosophy, history and poesy. He relegated the other, non-literary, activities of fantasy, like music and painting, to the background.

Hobbes retained the same distinction of faculties, but his psychology was profounder. He recognised that judgment as well as fancy is necessary in the composing of works of art, and that a ready fancy is of great use in philosophy as well as art. In a great poem, he thought, the framework of fact and principle is supplied by judgment, the imaginary events and hypotheses by fancy. Hobbes did not actually infer from this that judgment is a right selection of the materials supplied by fancy or wit, and because of his failure to do so, English literary criticism never reached a more universal theory of artistic production. At any rate, Temple and Dennis did not; and Dryden cut the Gordian knot by defining wit as 'thoughts and words elegantly adapted to the argument'—a definition without the least philosophical basis, and a plain misunderstanding of Hobbes.

(B) *The Psychological Period* (1690 +).

Locke's *Essay on the Human Understanding* was an account of how ideas are formed through experience. For him, wit was the ability to associate ideas, judgment the ability to distinguish them accurately. This neat distinction proved useful both to artists and critics. The thinkers who followed Locke, of whom Addison, Shaftesbury, Hutcheson, Hume and Burke were the chief, accepted his method and enlarged the sphere of æsthetic inquiry so as to include in it not only literature, but all agreeable cognitive experiences.

In his essays on *The Pleasures of the Imagination*, Addison took up the problem from the standpoint of the pseudo-Longinus treatise *Περὶ Ὕψους*, attributing such pleasures to the beauty, novelty or sublimity of the ideas apprehended. He had little to say about the distinction between them, and nothing at all about the ultimate reasons why they are pleasant.

Shaftesbury devoted his whole energies to the discovery of that ultimate reason. The general drift of his argument is clear, though it is often overlaid by side-issues. Judgments of æsthetic value are of the same sort as judgments of moral value, but on a lower level. Beauty implies design, and all designs are subordinate to one universal moral design. We begin by seeing value on the lower, material plane, where it takes the form of order, fitness and regularity. We ascend from this to the animal world, and thence to that of voluntary agents, where value receives the name *goodness* or *virtue*. Signor Rossi believes that Shaftesbury came to hold these views largely through dissatisfaction with the moral vacuum in Locke's teaching, but partly also because he was very familiar with the writings of the Stoic philosophers Epictetus and Arrian.

Hutcheson attempted to show by a systematic empirical investigation that the things that seem to us beautiful have certain characteristics, like variety in unity, which make them of special value in a well-planned universe. He thought that the pleasure our internal

sense derives from contemplating such objects was due to forethought on the part of our Creator, but he had the greatest difficulty in reconciling the existence of any such benevolent design with the existence of vulgar and vicious pleasures.

Hume treated beauty as a causal characteristic applicable to objects that give rise to pleasant experiences, where the pleasure is due to an agreeable emotion aroused either directly by the idea of the object, or indirectly by ideas associated with that idea. In other words, it is the pleasantness of the ideas that adhere to a percept that render it beautiful. Sympathy bridges the gulf of egoism in such associations, so that my idea of your pleasure becomes my pleasant idea of you. Art transforms impressions with unpleasant associations into beautiful experiences by associating them with more pleasant ideas, such as metre, rhyme, dramatic fittingness, or a skilfully-constructed narrative. ' This is the dominating motive of Hume's unsystematic investigations—the sentimentality of the beautiful. It implies then, first of all, a passage from wit to sentiment and then a reduction of the sensation of the beautiful to the sentiment of the beautiful ' (p. 71). When one encounters such a riot of psychologism, one cannot help sharing Mr. Karl Britton's feeling that ' justice has not been done to the cognitive powers of that part of the anatomy that occupies the seats of chairs ' (MIND, 1946, p. 363).

The one British contribution to æsthetics which is indubitably a work of genius is Burke's book *On the Origin of our Ideas of the Sublime and the Beautiful*, which, although published in 1756, clearly belongs to the psychological period. Burke started with the Addisonian trilogy, novelty, sublimity and beauty, but quickly convinced himself that the qualities that add impressiveness to a beautiful object are not in themselves pleasant, and that it is only when they are presented in a remote way that they produce desirable experiences. This desirableness, to which he gave the name ' delight ', is analysed by him as relief from pain, not positive pleasure. Burke applied this analysis in terms of two variables—excitement and pleasure—to the whole range of cognitive experiences, and he concluded it with a psycho-physical hypothesis as to effects of such experiences on the nervous system. Sublime experiences, in his view, stimulate the nervous system into activity ; their cessation therefore brings relief. Beautiful experiences soothe the nervous system into inactivity ; their cessation therefore brings irritation. A still more daring hypothesis is put forward as to the physical properties of the objects which give rise to sublime or beautiful experiences. The objects of the former are large, or, at least, larger than the subject expects ; the objects of the latter are small, or, at least, smaller than the subject expects. Burke concluded his treatise with a brilliant section on the use of language as a means of exciting the imagination and the emotions, a section which completely superseded everything that had been said up to that time on the psychology of speech.

(C) *The Technical Period* (1750 +).

Hogarth's *Analysis of Beauty* set forth a theory of the nature of beauty in terms of surfaces and perspectives, which stemmed from Michelangelo's speculations on the line of beauty, but contained much that was original. The idea that a well-composed picture must have the formation of a cone with undulating sides and with cross-sections in the proportion 1 : 2 : 3 is, of course, too concrete for æsthetic theory, but seems to have proved a valuable rule of thumb.

Reynolds' writings on art, which began about 1759, reveal a refined and sensitive, rather than an original, mind. He believed that there was a typical beauty for each species, that it was necessary in art to pass beyond the imitation of nature to idealised types and dramatised attitudes, and that an artist must be subservient to the best in established conventions. In Reynolds' dignified prose, these views sound moderate and sensible. Taken as practical precepts, however, they led inevitably to the horrors of the 'Brown Soup' period in British art. It is hard not to believe that Ruskin was right in preferring the impressionistic realism of Turner to the meticulous classicism of the Poussins.

Lord Kames's *Elements of Criticism* revealed the same technical spirit in the field of literature. He tried to avoid philosophical issues, but gave an acute account of how different literary and dramatic effects are produced. In opposition to this technicalism, there arose the cult of the picturesque, a movement in which Rousseau and Uvedale Price played a notable part. It was not altogether their fault that the Romantic Movement in art slipped down the by-path of the Gothic Revival, undoing in its disastrous developments most of the æsthetic achievements of the Age of Reason.

(D) *The Inductive Period* (1780 +).

Hume's success as an iconoclast convinced the best philosophers that the general adequacy of human faculties for their proper ends must be taken as a datum in philosophical inquiries. The Common-Sense School prided itself, not on meeting all possible doubts, but on considering the facts, all the facts, and nothing but the facts. From 1785, the date of the publication of Reid's *Essays on the Intellectual Powers*, until 1811, the year in which Jeffrey reviewed the second edition of Allison's *Essay on the Nature and Principles of Taste*, this inductive attitude to the problem of beauty held its own in the domain of philosophy. Reading the sober, balanced periods of Stewart, Allison and Jeffrey, it is impossible not to feel that these questions were then much better understood and more fairly discussed than at any subsequent period. But, of course, Jeffrey and his friends were already menshevising, *i.e.*, writing for a minority. The Romantic Movement diverted the interest of careful readers back to historical questions. The essays of

Macaulay, Jeffrey's true successor in the Whig tradition, never reached the level of abstract thinking, while Benthamism with its famous principle 'Push-pin is as good as poetry' was a direct and deliberate negation of æsthetics.

Signor Rossi here gives extracts from Dugald Stewart and Jeffrey, but Jeffrey's essay is itself the best epitome of the movement. Great analytic thinkers, like Prof. Broad, who find æsthetics boring, and suspect it to be bogus (MIND, 1941, p. 198), should read this remarkable review, which is available in a volume published in 1843 under the title *Contributions to the Edinburgh Review*. Jeffrey's conclusions, which are substantially the same as Allison's, deserve notice even in these Surrealist times, because they anticipate by sheer honest induction one of the fundamental doctrines of the Existentialists on the origin of affects. (1) Beauty is a characteristic of perceptual objects, not sensations. There are no intrinsically beautiful sensations. (2) Those, and only those, perceptual objects are beautiful which recall past sensations or percepts that were both pleasant and interesting. The word 'interesting' excludes orgies of organic sensation, like a hot bath, and if Jeffrey had paid more careful attention to it, he would perhaps have been able to define the conditions under which an object appears beautiful. As it is, he never got beyond describing the psychological act of appreciation.

Can we add anything, after a hundred years of experimental psychology, to Jeffrey's account of beauty? With the help of Russell, Moore and Broad's analysis of sense-data, we can at least state his point of view more clearly and completely. (1) The objective constituent of an æsthetic experience must evoke attention by contrast with what surrounds it in space-time. (2) The degree of pleasure experienced must be proportional to the extent to which the subject discriminates, or seems to discriminate, the region of space-time occupied by the objective constituent. (3) Accepting Ward's doctrine that the degree of differentiation in a perception is proportional to the degree in which it is assimilated to past perceptions, it follows that its hedonic tone will be determined by that of these revived experiences, and therefore by the pleasantness of past perceptions. The situation is as if one were forbidden to eat buns on their first appearance at table, but could eat them subsequently provided there were also new buns on the table. (4) An æsthetic experience must be conclusive. There must come a time when the cessation of perceptions will add more to our appreciation of the object than their continuation.

Before taking leave of this interesting book, the reviewer would like to comment on two erroneous opinions, of which Signor Rossi makes repeated use in his introduction and notes. The first of these is the proposition that British æsthetic theory never succeeded in emerging from the charmed circle ('circulo fatato') of topics and examples in which it was born (p. 9). To this one would reply that all creative thought must necessarily take place in a charmed

circle of friendship and mutual understanding, so that the only issue is whether British æsthetic theory ever achieved any results that would stand the common light of day. On that point, no one who reads Jeffrey's essay, or is familiar with the beautiful city that he helped to create, will have any doubts. The second proposition to which one takes exception is the opinion that examples prove nothing in the moral sciences (pp. 12, 69). No doubt, literary allusions to Rafaello's Vatican frescoes or Beethoven's Fifth Symphony prove nothing in a scientific sense, but people who take their æsthetics seriously do not settle disputed points with literary allusions. They insist on seeing or hearing the works about which they hold different views from their friends. For the ordinary man æsthetics is a probative, if not an experimental, discipline ; for the artist, it is both probative and experimental.

ARTHUR T. SHILLINGLAW.

The A Priori *in Physical Theory.* By ARTHUR PAP. New York: King's Crown Press; London: G. Cumberlege, 1946. Pp. x + 102. 13s. 6d.

THIS is an interesting and useful piece of work on an important subject. The author has been greatly influenced by the writings of Poincaré, and the passage from *La Science et l'Hypothèse* quoted at the beginning of the preface gives the underlying idea of the book: "Les principes sont des conventions et des définitions déguisées. Ils sont cependant tirées de lois expérimentales, ces lois ont été pour ainsi dire érigées en principes auxquels notre esprit attribue une valeur absolue." This must not, however, be taken to imply that the work is unoriginal; for although the underlying idea is Poincaré's, its development and applications are independent. The second part of the work, concerned largely with certain aspects of Newtonian mechanics, seems to me to be especially useful and to contain much that is acute and illuminating. It is, therefore, a great pity that the book is so short and that such a large part of it is taken up by quotations from other writers: as a result, many important questions are discussed very sketchily, the connexions between the different points Mr. Pap is making are not always easy to see, and in places the argument suffers in clarity from over-compression. I hope that when publishing conditions become easier Mr. Pap will find it possible to discuss the subject in more detail and at greater length.

I think that what Mr. Pap says about the transformation of empirical generalisations into definitions is both true and important. This process was emphasized by Poincaré and also by V. F. Lenzen. For instance, to quote one of Lenzen's examples, the law expressing the dependence of the stress in an elastic body upon the strain,

originally an inductive generalisation, is transformed, at a later stage of scientific development, into a definition of elastic constant; and similarly, to quote another of his examples, the law stating the functional relation between the length of a wire and its temperature is transformed into the definition of coefficient of linear expansion. A word of caution is, however, necessary, and Mr. Pap very properly provides it. It is, strictly speaking, self-contradictory to say of one and the same proposition that it is an empirical generalisation at one time and not an empirical generalisation at another time: for (I am putting this in my own words) "being an empirical generalisation" is an *internal* property of a proposition, and hence if *p* is an empirical generalisation, *it* cannot be "transformed" into something which is not an empirical generalisation. (Incidentally, Mr. Pap seems to suggest, if I understand him rightly, that whether or not "being an empirical generalisation" is what I have called an "internal" property is a matter of choice or decision. I do not think that this is true, but the point does not affect the main argument.) We can say, however, that a *sentence* which is at one time used to express an empirical generalisation may, at another time, be used to express a definition, though of course the meaning of the sentence would change in the process. And this, as Mr. Pap points out, is by no means trivial. For, to quote his own words, "the sort of redefinitions which we, following Lenzen's theory of 'successive definition', refer to, are not arbitrary but grounded in empirical discoveries. In particular, we are referring to redefinitions which leave at least part of the extension or denotation of the redefined term unchanged: what is changed are not so much the objects conceived as our ways of conceiving of the (numerically) same objects" (p. 25). The specific examples of this process of "successive definition" which Mr. Pap gives, as well as his general discussion of the subject, are both interesting and enlightening. It seems to me, however, that a very important fault of the book is that Mr. Pap does not discuss in exactly what sense, if any, definitions (or "linguistic conventions") can be said to "give rise" to necessary propositions, or as he puts it, to analytic truth: this should, of course, constitute one of the chief topics in any satisfactory account of the nature of logical necessity; and the lack of such a discussion in Mr. Pap's book not only produces a certain amount of unclarity in his treatment of "successive definition", but also gives one the feeling that a fundamental aspect of the problem of the *a priori* (which, after all, occupies a very prominent place in Mr. Pap's essay quite apart from its applications to physical theory) is passed over, and that the author's views on the subject are not really made clear. It is true that his remarks on "real" and "nominal" definition and on definition by postulates bear on this question and might, if developed, throw some light on it. But at present they are too brief and disconnected to be of much help.

In addition to this conception of "successive definition", there

is another conception on which Mr. Pap lays very great stress throughout the work, namely the conception "of the functional" *a priori*, or as he sometimes calls it, "functional analyticity". I am not certain that I have understood all that Mr. Pap says about it, but so far as I can see the chief point comes to this. Mr. Pap argues that if we concentrate on the division of propositions into "formally" analytic and "formally" synthetic, we shall fail to understand the logic of actual scientific procedures. For in science, he claims, a proposition which on "formal grounds" must be classified as synthetic may yet, in certain contexts, "function analytically". For instance, suppose that what appears to be an isolated body is observed to traverse unequal spaces in equal times; and suppose that we don't take this as counting against the law of inertia, but conclude that the body in question, though it *seemed* isolated, was not really so. In this case, if I understand Mr. Pap rightly, the law of inertia functions analytically; yet it is not, on that account, *formally* analytic. Now, by saying that a proposition is formally analytic, Mr. Pap appears to mean that its contradictory is *self*-contradictory; and he is therefore pointing out that it is not only those propositions which are analytic in this sense that can perform a "prescriptive" or "regulative" function in empirical inquiry. This sort of function can be, and often is, performed by propositions the contradictories of which are not self-contradictory, that is, by propositions which are formally synthetic. Further, it won't do to say, as for instance C. I. Lewis has said, that "that is *a priori* which we can maintain in the face of all experience, come what will". To quote Mr. Pap's criticism, "Now, 'can', in the statement quoted above, may refer either to logical possibility or to practical possibility. But in whichever of these two senses it be taken, Lewis' statement would hardly retain its plausibility if one were to substitute 'analytic', in the sense of formal logic, for 'a priori'. Since, as Duhem has conclusively shown, it is never an isolated empirical hypothesis, but always an entire set of hypotheses that are submitted to experimental test, it is always *logically* possible to adhere to a given member of that set and to blame the failure of the deductive consequences to be verified upon the other hypotheses involved in the disappointed prediction; but such a possibility does not make an empirical hypothesis analytic. And if the possibility referred to is *practical* possibility, it would be extremely complicated to decide whether a given statement is analytic; at least this could not be decided by the methods of formal logic" (pp. 4-5). Yet there is *some* connexion between "formal" analyticity and "functional" analyticity (quite apart from the fact that the former is, of course, a sufficient condition for the latter). For the greater the scientist's tendency to use an empirical law analytically, the greater will be the likelihood of the suitable redefinition of the relevant terms and the consequent "transformation" of the law into a formally analytic proposition. (I am putting this rather vaguely

since, as I have mentioned before, the relation between "linguistic conventions" and the analytic truths to which they are said to "give rise" is not discussed.)

This is as good an account of the various things which Mr. Pap says about this conception of "functional" analyticity as I am able to give in a short space, and I hope it is not too inaccurate or misleading. Now, I think that this conception, and the examples Mr. Pap gives to illustrate it, do draw attention to an important feature of scientific procedures, and I think, therefore, that his discussion is valuable; but I feel rather doubtful how far it can really be said to improve our grasp of the nature of necessary connexion. And I should like to state, as briefly as I can, one of my reasons for saying this. I have quoted the passage in which Mr. Pap criticises Lewis's statement that the *a priori* is that which we can maintain in the face of all experience, come what will. But now, suppose someone claimed that whilst it is wrong to characterise the *a priori* in precisely the terms which Prof. Lewis uses (and which Mr. Pap calls "pragmatic"), it is possible to do so, and to do so rightly, in somewhat different terms. For instance, suppose someone claimed that a proposition is *a priori* if, and only if, it is not, even in principle, subject to empirical disconfirmation; that is, if, and only if, it is *logically* impossible to disconfirm it by empirical evidence. (The expression "not subject to empirical disconfirmation" must, of course, be understood to mean "not subject to even partial empirical disconfirmation.") Now, I am not saying that this would be a useful or enlightening *definition;* but the argument which Mr. Pap gives against Lewis's statement would not, so far as I can see, hold against this one. For whilst from the fact that it is logically possible to maintain p in the face of all experience, it does not follow, as Mr. Pap points out, that p is not an empirical hypothesis, from the fact that it is logically impossible to disconfirm p by empirical evidence, it does, I think, follow that p is not an empirical hypothesis, but is, in some sense, *a priori*. And conversely, from the fact that p is not an empirical hypothesis, it follows that it is logically impossible to disconfirm p by empirical evidence. In fact, Mr. Pap himself seems to agree to the latter. For, in the part of the book in which he attempts to show that a proposition may function analytically without being formally analytic, he says, "Here again, laws of nature exercise a prescriptive function, even though they first had to be inductively confirmed and are in principle subject to empirical disconfirmation" (p. 33). He thus himself introduces this expression "being in principle subject to empirical disconfirmation", and seems to imply that from the fact that p is in principle subject to empirical disconfirmation, it follows that p is not formally analytic (though not, of course, that p cannot function analytically). And, obviously, if this is so, then from the fact that p is formally analytic, it will follow that p is not, even in principle, subject to empirical disconfirmation. But now, would Mr. Pap also maintain that if p is not

in principle subject to empirical disconfirmation, it follows that it is formally analytic? That is, would he maintain that the proposition "*p* is not in principle subject to empirical disconfirmation" is not only a necessary but also a sufficient condition for the proposition "*p* is formally analytic"? Perhaps he would; but if he did, he would be making a claim which many philosophers would dispute: for they would say, I think, that some propositions which are not, even in principle, subject to empirical disconfirmation are yet not *formally* analytic, in the sense that their contradictories are self-contradictory. Yet if a proposition is not in principle subject to empirical disconfirmation, it does seem to follow that it is *a priori*. Hence, such philosophers would maintain that there is a sense of "*a priori*" which is different from "formally analytic" and also from "functionally analytic". Now, Mr. Pap may claim that the propositions with regard to which we feel tempted to suppose both that they are not formally analytic and also that they are in principle incapable of empirical disconfirmation are all, in fact, either empirical propositions which function analytically, or inductive generalisations which have been transformed into definitions, or "imperatives addressed to scientific procedure". For he argues, in the second part of the work, that this is the real status of what Kant took to be the synthetic *a priori* principles of Newtonian mechanics. I am afraid, however, that to suppose that all the propositions answering to the description I have just given are either formally synthetic propositions which function analytically or inductive generalisations which have been transformed into definitions, seems to me to be unsatisfactory; and as I can attach no clear meaning to the word "imperative" when it is said that the principle of causality is an imperative addressed to scientific procedure, I am unable to see what the remaining alternative really comes to.

This review, though already long, does not, by any means, touch on all the suggestive remarks which Mr. Pap makes in the course of the book. But I should like to mention also the last chapter, on "Idealisation in Physics", which includes an interesting, though rather brief, discussion of the important problem of interpreting contrary-to-fact conditionals.

C. LEWY.

V.—NEW BOOKS.

The Principles of Moral Judgement. By W. D. LAMONT, M.A., D.Phil. Oxford: At the Clarendon Press (Geoffrey Cumberlege), 1946. English price 15s. net.

THE author explains his aim and his view of ethics in the preface and chapter i: "The central aim of this book is to examine the common moral consciousness with a view to discovering the principles or standards in accordance with which moral judgements are made." "I am inquiring not about standards which 'ought' to be used in our judgements of right and wrong, but about those standards which 'are in fact' used by us in our moral judgements" (preface). The point which Lamont wants to emphasise is that such an inquiry into 'facts' must use a 'scientific' procedure, *i.e.*, one which works by means of observation of facts, hypothesis and verification. He tells us that he was made aware of the possibilities of such a method by the work of Piaget and Sharp (who respectively investigated the moral ideas of children and of students). Now the ideas of children and students may furnish useful material for the educator, but hardly, one would think, for a science of ethics, unless indeed we think, with Lamont, that "all moral judgements are relevant to our inquiry—those of plain, foolish, and even vicious men, equally with those of the educated, wise, and virtuous" (p. 20). The word 'equally' in such a context is ambiguous, but the moral judgements of foolish and vicious men would surely have no ethical value at all. "It is not our business to assume from the outset that certain judgements are, in fact, true", says Lamont; but if we are not to assume provisionally that the accepted moral ideas are true, how are we to make a start at all?

Chapter ii deals with the Authoritarian theory of morality "in its most common form", *viz.* that the ultimate standard of right and wrong is the Will or Command of God (p. 44). The chapter is not of great importance for the author's general argument, and I would only remark that Lamont's criticism of the theory is probably too general and abstract to satisfy those who accept the conception of a Christian ethics.

The author attaches "most importance to chapters iii, iv, and v on The Morality of Social Justice, where I attempt to exhibit the principles which I believe to be fundamental in the idea of moral obligation" (preface vi). I will state some of the main contentions of these chapters and comment upon them. Chapter iii discusses Rights, Duties and Obligations. Lamont holds that Duties are consequent upon Rights. This seems to me a mistake. He also holds that duties are correlative to rights; but things that are strictly correlative can hardly stand in a relation of antecedence and consequence (unless in a special case such as premisses and conclusion). The relationships husband and wife are strictly correlative, and one is not consequent upon the other. If rights and obligations are strictly correlative, we should expect them to rest on the same basis, as, *e.g.*, in Green's *Political Obligation* they are held to rest on the idea of a common good. Now Lamont himself holds that rights "must be defined in relation to interests" (p. 95), but he seems chary of using the idea of a common good, and objects that if the common good is made the formal principle of obligation we are forced to hold that rights

are logically consequent upon duties (p. 123). To me, of course, this seems as mistaken as the converse view. He further objects that, if the common good is our principle, we should expect a preference to be given to other-regarding ends over self-regarding ones, whereas law seems actually to lay stress on self-regarding ends, when it asks to be shown what interest a person has in a litigation. But this seems only to mean (as Lamont, I think, is aware) that the law wants to be shown that the litigant is concerned in the case in some relevant way (has some *locus standi*) and is not intruding himself into a case with which he has no real concern.

Perhaps Lamont doubts the extent to which the good can be common; he is more ready to speak of a conciliation of interests than of a common good. And this again may be connected with his insistence that the good for A may be different from, and may conflict with, the good for B. He defends this personal or subjective reference of the good (I think successfully) in the following passage: "Certainly, if the value judgement 'This is good' be true, then the judgement is true for everyone; but the thing is not necessarily therefore good for everyone. In the same way, if the judgement 'This is a perfect fit' be true, it is true for everyone, but the thing to which it is applied may be a perfect fit for me though not for you". But although Lamont 'works on the hypothesis' that good 'necessarily carries some kind of subjective reference', 'some essential reference to will or desire' (p. 97), he rejects the definition of good as the object of desire; 'that is not', he says, 'the theory which I hold'. But it is not easy to be sure what exactly the theory which he holds is; for on a later page (119) he tells us that 'an end means some object or state of affairs desired by a conative subject'. But, at any rate, he shows no great favour to the idea of a common good. He allows that there may be a 'coherent pattern of ends' or 'total good' for an individual. "But the system of these personal goods is not a good, any more than the system of persons is a person.... Consequently a common good is not an all-embracing system of the systems of personal goods, but is, so to speak, the collection of bridges which join the various personal goods, these bridges being the multitude of ends and means which are included in the system of each person concerned. Without such bridges there is no 'good for us', but only 'good for you' and 'good for me' (p. 116)".

From what has been said we can see why Lamont, when he comes to sum up his view of Social Justice, lays great stress on the two principles of Liberty—the liberty of the individual, in the sphere of action which his rights allow him, to pursue what he deems to be his own good—and Equality—the equal opportunity of all individuals to do this. A third principle, called here Merit, but better known perhaps as the Platonic principle of Fitness for the discharge of a particular function, is recognised, but not regarded as equally fundamental, since it implies relationships within an an organised group.

Chapter vi is entitled "Ideal Morality". Lamont reminds us that Bosanquet in his Gifford Lectures has characterised the moral world as the world of claims and counter-claims. The question is thus suggested whether there is a morality which transcends this world of claims. Lamont holds that there is, since we can renounce our own rights or claims; but he adds that it does not follow that we can ignore the claims of others. "Ideal morality does not abolish justice" (p. 171). I am inclined to agree with Lamont that Bosanquet in the chapter referred to was unduly influenced by Bradley. Lamont does not mention Bradley by name; he

speaks rather of Hegelian influences. Hegel's influence, however, is more apparent in Bosanquet's book on the *Philosophical Theory of the State*; the influence of *Appearance and Reality*, I think, came later.

Chapter vii is on "Moral Standards and Human Nature". The author's aim is "to show that the standards which we have described are grounded in certain fundamental characteristics of human nature" (p. 179). This recalls what he had said incidentally in an earlier chapter about Kant's categories, *e.g.*, substance and causality, as "grounded in the nature of the human mind" (p. 12). This seems to me to point to a wrong psychological interpretation of Kant. On the truer interpretation the categories are necessary if our experience of the world is to be intelligible. Similarly, if certain moral principles are required to make moral experience intelligible, there is no need for an additional grounding in human nature. The chapter seems to me to contribute little to the general argument.

There is one passage in it, however, which calls for comment. Lamont repeats the familiar charge against Mill of committing an obvious fallacy at the beginning of chapter iv of *Utilitarianism*, where the general happiness is said to be a good to the aggregate of all persons. It is more than time that this charge was dropped by our ethical books. Since the publication by Elliot of *The Letters of John Stuart Mill* we know that Mill himself repudiated this misunderstanding of his words; but indeed the misunderstanding should never have occurred, for the careful reader of *Utilitarianism* can see that in chapter iv Mill is concerned only with the nature of human good or happiness; he had already, in chapter iii, dealt with the influences which can be brought to bear on the individual to make him consider the general happiness and not merely his own.

The last chapter discusses Responsibility, and incidentally the theory of punishment in its bearing on responsibility. It is maintained that indeterminism is incompatible alike with the intelligibility of human behaviour and with responsibility. Lamont distinguishes between mechanical and teleological causation, but it is perhaps questionable whether he is sufficiently alive to the danger that teleological causation may itself be construed on mechanical lines; motives may be treated as external forces operating on the will.

The foregoing notice may have already suggested some of the merits of Lamont's book. He does not repeat the usual topics and arguments of the text-books, but tries to see the subject freshly for himself. His use of legal material rightly emphasises the relationship of morality to law and of ethics to the practical problems which the law has to decide. I have found the book very interesting, and it is well and clearly written. As he tells us in the preface, Lamont does not "pretend to offer a complete system of ethics", but he has at any rate produced a book which should stimulate the interest of the student of ethics in his subject.

H. BARKER.

Preface to Philosophy: Textbook. By HOCKING, BLANSHARD, HENDEL and RANDALL (Jr.). Ed. TOLLEY. New York: The Macmillan Co., 1946. Pp. x + 508. $3.00.

THIS joint work is intended to be read in conjunction with a companion volume of 'readings' culled from the classics of ancient and modern philosophy. The textbook is not a *commentary* on the readings but an

independent discussion of philosophical questions which the readings serve mainly to illustrate. A system of footnotes connects the two volumes for purposes of cross-reference.

It is claimed in the preface that a 'careful study of the text and readings' should help to resolve the 'conflict of values and ends' to which much of the confusion of contemporary life is due. This strikes the key-note of an intended relevance to the 'present crisis of civilisation' which underlies the whole project. And the fact that the crucial issues from this point of view are of an ethical and religious character has considerably limited the scope of the *Textbook* as an introduction to philosophy. The authors have little to say of logic, aesthetics or epistemology. Their subject is Man—man as "concerned about his origin and end, about his purposes and goals, about the meaning of life and the nature of reality" (p. v).

The book is divided into five Parts. In the first Part, entitled *What is Man?*, W. E. Hocking opens with a survey of the situation which sets the problem of the book—the situation of man as a 'unique creature' placed 'in the midst of an infinite universe'. This is followed by three Parts in which are examined 'several phases of the kind of life man has worked out, ethical, political and religious': Part II, by B. Blanshard, on *Personal Ethics*, Part III, by C. W. Hendel, on *Social and Political Philosophy* and Part IV, by J. H. Randall (Jr.), on *The Meaning of Religion for Man*. Finally, in the fifth Part, entitled *A World-View*, Hocking returns to consider 'the universe itself, the total environment of all this human thought and enterprise'. There are no chapters, but each Part is divided, under appropriate subheadings, into sections and subsections.

Hocking's two main themes in the first Part are (*a*) man's capacity for self-criticism implying an awareness and application of 'standards of judgement' and (*b*) his consequent creativity. Physically 'mediocre, nondescript and unfinished', man is more than a mere animal. He is an 'animal *with a soul*'. And 'soul' implies self-transcendence, a capacity to think of oneself and of things 'in the frame of the whole in which they are placed'. "*The soul is the self of man engaged in getting its bearings in the total universe*" (p. 17). Religion is an aid and encouragement in this task. It relates man reassuringly to the 'totality with which he is destined to deal'. But the reassurance which religion gives brings with it an obligation. Those who are assured of the essential rightness of things must acknowledge a 'right way of living' which it is their duty to find. And this implies a reference to standards of right thinking and doing. A standard is a requirement, extracted from past experience, which the human mind sets up as 'a measure for its own current achievement'. Requirements thus empirically derived "*run to their own limit as ideals* which exercise their command without regard to special social circumstances" (p. 45). As such they operate as standards of self-correction in thought, taste and conduct. But self-correction implies self-awareness, and both imply a self-transcendence which is only another name for freedom. The freedom of the human mind shows itself in its *creativeness*. Causal processes cannot hesitate. But man *delays his decisions* not, chiefly, from hesitating between given alternatives, but because he wants time to devise 'something which will suit him better'. This means that he is creative. It means that he is free to employ the regularities of nature to 'produce something different, something of which nature had not dreamed'. And man is faced by the 'total duty or mission' of exercising this creativity upon both himself and his world. The achievement

of civilisation is the progressive 'self-creation of man through the shaping of his social environment'.

Written on the usual lines, but with considerable freshness of analysis and illustration, the second Part consists of a survey of the classical problems of Ethics. It is through the failure of 'common-sense' either to pronounce at all or to make consistent pronouncements on questions of conduct that ethical reflexion becomes necessary. And its chief task is to determine what makes an action good or right. Dividing the main types of answer to this question into 'act-theories' and 'end-theories', Blanshard disposes of such act-theories as (*a*) that the rightness of an act is immediately intuitable, (*b*) that it is solely a question of rules, and (*c*) that it is a matter of attitude, by showing that, in various ways, these theories come to grief by *neglecting consequences*. Then, after a vigorous discussion of the case for and against Utilitarianism, in the course of which he discloses his preference for end-theories, he combines the notions of 'ideal end' and 'rule' in the formula that whatever we perceive to be our duty we, at the same time, perceive to be a course of conduct which is likely to further the greatest good of the whole.

I found the inquiry into social and political questions the least satisfactory section of the book. It seems to me that what Hendel has to say could have been said with far less prolixity and platitudinous repetition. And it is here too that the dubious intention of making philosophy the text of a sermon for the times is most in evidence. In the course of the argument, principles of 'ideal democracy' derived from Plato's *Republic*, the Gospels and Rousseau are applied to the contemporary problems of nationalism, class-war, international relations, etc. The whole discussion reflects the self-assured optimism of a land which can still afford the luxury of a free, undoctored capitalism. There is facile talk of a 'community dedicated to freedom and equality' as if it were not being increasingly discovered all over the world that the achievement of genuine social equality is tragically irreconcilable with the preservation of political freedom.

Religion, we are told in Part IV, may be approached both from within and from the outside—as 'something to be seen by an observer'. Viewed objectively, it is a 'social enterprise' which performs "the three fundamental functions of . . . *celebration*, the social observance of values to which the group is devoted, . . . *consecration*, the co-operative dedication to those values, and . . . *clarification*, the reflective criticism and appraisal of their significance and worth" (p. 319). Viewed from within, the *deeper* function of religion is 'salvation', but it also offers to 'the average man for whom it is one important concern among others', fellowship, guidance and faith. In a concluding analysis of fundamental religious beliefs the plea is made that these should be regarded as *symbols* to be "judged, not by their literal truth or correctness as a series of propositions about facts, but by their adequacy and effectiveness in performing their *religious* function of strengthening faith" (p. 375). As such they "offer no explanation of anything in rivalry to science or philosophic inquiry; they furnish data for such inquiry, not competition with its conclusions" (p. 380). There is room, however, for intellectual explanations of religious symbolism. For these, in turn, may become 'symbols serving the faith in the Divine' and thus 'acquire a genuine *religious* function. But not only is there no conflict *from this end* between religion and science. Scientific theories are themselves "symbols employed in the practice of scientific investiga-

tion, whose validity depends upon their fruitfulness in guiding inquiry rather than on their literal 'truth'" (p. 389). There has only been conflict between new scientific knowledge and an outmoded *science* enshrined in religious beliefs.

The last Part is an essay in speculative metaphysics. The empirical method of moving from part to whole, Hocking claims, cannot of itself yield a 'world-view'. Apart from the order of *classes* which is chiefly of logical significance, the world displays an order of *causes*, which is the 'primary care of science', and an order of *purposes* which, unlike the causal order, runs from the future to the present and occurs only locally and sporadically, that is, only where there are minds. If, however, we could suppose the emergence of mind to be the purpose served by nature as a whole, instead of these fragments of human purpose falling within a totality of *mere* cause and effect, the whole causal order would at once become a 'total purposive order' with only local interference with its causality, and consequently without prejudice to the scientist's over-all picture of the world. Is there, then, a world-view which would enable us to relate 'causality' to 'purpose', or matter to mind, in this way? Rejecting both the traditional dualist solutions and any monism which would make mind a mere adjunct of matter, Hocking finds the desired solution in a version of Berkeleyanism. Berkeley, he contends, was right in thinking that 'nothing can act on a mind but a mind' and that sensation, therefore, is a *communicated* experience; but wrong in supposing God to be an 'object of theory' which had to be brought in to account for sensation. God is *directly experienced* in the 'inescapable factor of outer action' which we experience as the other side of our passivity in the reception of sense-impressions. We are, in consequence, sure of ourselves and of God 'in the same breath' and of nature as a region of shared experience between us. This secures the inclusion of 'matter' in 'mind' and sets causality within a total purpose. For it is pointless to duplicate nature by giving it "a separate existence, in addition to the common awareness-of-nature in the mind of God and the minds of men" (p. 473, n. 1).

But does this, as Hocking claims, secure the *objectivity* of nature? He supposes that, in Berkeley's world, "all of space is there and all its contents, just as we perceive them" (p. 469). If, however, the *esse* of a given percept is to be perceived *by the mind which perceives it*, the fact that God perceives an object which I am not perceiving does not mean that the *object* is 'there' in the mind of God waiting to be perceived by me. For *I* am not an 'idea' and, therefore, not in the mind of God. What it means is that *God* is always there waiting to produce telepathically in me *another* object which, while it duplicates the one He perceived, can only come within *my* purview and must accordingly be private to me. "This physical scene", Hocking writes, "is *not my world alone*, but a world shared with at least one other mind, that of the giver" (p. 473). The correct inference from the New Principle, however, is that I share with God and my fellows the perception of worlds which are qualitatively similar. Either the *esse* of nothing whatever is *percipi*, in which case Berkeleyanism falls to the ground, or *this* physical scene is, of necessity, my world alone.

J. R. JONES.

Le Jugement d'Existence chez Aristote. Par SUZANNE MANSION. Louvain, Institut Supérieur de Philosophie, 1946. Pp. xiv, 312.

DR. MANSION'S aim is to make clear what part judgements that something exists play in science according to Aristotle. That they ought to play some part appears both from Aristotle's view that there is no reality beyond existing things, so that science to be about anything real must be about existences, and also from his explicitly including 'whether it exists' among the questions that the scientist asks. On the other hand, Aristotle's account of the nature of science is so largely Platonic that it gives the impression of being just as aloof from individuals as the dialectic described in the *Republic*. He says there is no science of individuals; he also says that only individuals exist; it seems to follow that science is not concerned with existence.

Dr. Mansion makes long and thorough preparations before answering her problem, so that her book becomes a substantial exposition of large portions of Aristotle's theory of science, and a substantial commentary on several chapters of the *Posterior Analytics*. She devotes a chapter each to science as knowledge by the cause, science as knowledge of the essence, science as knowledge of the necessary, science as knowledge of the universal, opinion, the principles of science, and demonstration, before coming in her last three chapters to Aristotle's theory of being and the place and significance of the existential judgement in science as he conceives it.

I translate her summary of her answer as follows. 'Science according to the master is concerned with the real. Hence, in order that his propositions may lay hold on being, the thought of the scientist must enter into contact with existing realities and affirm their existence prior to all deduction. However, this affirmation of existence is not by itself alone the subject of a distinct judgement. For it is impossible to know whether a thing exists without having some knowledge of what it is. Our primitive apprehension of the thing known is both essential and existential. And, as the quiddity of things constitutes the formal object of science, the sort of proposition that it must take as the basis of its speculations is a judgement about the nature of an existing thing: it is the "definition of the essence". Real definition implies, for the Stagirite, an act by which the mind recognises the existence of the object defined. If we make this act explicit, it consists in affirming that there exist in nature beings corresponding to the definition. The existence here concerned is that of concrete individuals, but it is reached in an abstract and indeterminate manner.'

The author opines that Jaeger's account of the development of Aristotle's thought cannot alter our conception of Aristotle's system, because his writings as we have them represent in the main his thought during the last period of his career. She does not mention Solmsen's application of Jaeger's point of view to the *Organon*, or the subsequent discussion by Solmsen and Ross in *The Philosophical Review*. She finds, for her part, that Aristotle's opinions changed in at least one particular as he was writing the *Posterior Analytics:* he began by affirming and ended by denying that definition is a term and not a premise. (I incline myself to think that he always thought of a definition as a premise and not a term. Her 'peremptory' references for the other view all seem ineffective to me. Can she have been misled by the ambiguity ὅρος = (1) definition, and (2) term? This is the only case in which Dr. Mansion's references seem failures to me. Elsewhere her documentation is always strong.)

The world of thought implied in the book is mostly French-speaking Catholicism. The logical scheme with which Dr. Mansion approaches Aristotle is that of Tricot's *Traité de Logique Formelle* and Maritain's *Petite Logique*. Maritain in this work is anxious to maintain that nothing both new and true has been said in logic since 1600. He regards nominalism as a 'taint'. He upholds Aristotle's syllogism as the form *par excellence* of all deductive reasoning. He rejects with skill and contempt the recent doctrine that 'all men are mortal' does not entail 'some men are mortal'. He distinguishes logic, which serves thought, from the new logistic, which would be a substitute for thought. Definition is to him primarily real definition of the essence. Nominal definition is secondary and derivative.

Thus the logic with which Dr. Mansion approaches Aristotle is Aristotle's own logic. Her criticism of him is all from the inside, and consists mostly in tidying him up. The most radical comment she makes is that he sometimes confused the plane of thought with that of reality. Those who are sympathetic to recent Protestant logic as well as interested in Aristotle's logic will therefore not find in her book that after-Russell approach to the logic of Plato and Aristotle which they hope to see some day. There is nothing here such as Ryle's article on Plato's *Parmenides*.

Although I believe that under the head of real definition men have sometimes had in mind true and useful questions, I do not believe that real definition as a statement of *essence* is anything true. I believe that the phrase 'the essence of x' can have only two true meanings: (1) it can be a long way of saying 'x', and (2) it can mean the same as 'what we mean by the word "x"'. In all other logical senses it is an error. The incredible obscurity of *Posterior Analytics* II and *Metaphysics* Z is due to the fact that in both of them Aristotle is pursuing with wonderful patience and ingenuity a myth—the myth of essence, the mysterious essence of x that is somehow different from x and yet the same as x. The question 'What is x?' must be resolved either into the request for a nominal definition, 'What does the word "x" mean?', or into the request for an analysis, 'What is x a complex of?'. There is no third possibility, 'What is the essence of x?'. That is what Aristotle nearly but never quite saw. To Plato definition was always purely real, a statement of essence (except for a passage at the end of the *Theaetetus*). To Aristotle in the early *Topics* it was still purely real. In *Posterior Analytics* II, wrestling with the problem to the last degree of strain, he comes up in chapter 7 against the idea that definition might be nominal, and shies violently away from it. In chapter 10 he lays down at the beginning that there is a sort of definition that is nominal; but in his list at the end of the chapter this sort has disappeared again. In *Posterior Analytics* I he seems quite calmly to regard definitions as not being assertions at all (chapters 2 and 10). (On this ground I should like to explore the possibility that the first book is later than the second.)

Consider the nominal definition,

(1) 'The word "triangle" means a threesided rectilinear figure',

and the real definition,

(2) 'A triangle is a threesided rectilinear figure'.

The first mentions the word 'triangle' and does not use it; the second uses the word 'triangle' and does not mention it. Or, as Dr. Mansion's logicians would say, the first *supponit materialiter*, the second *formaliter*. Dr. Mansion believes that only the second of these sentences attains reality. I believe that only the first does so. The first attains reality because it is

an assertion that a certain real word is really used by certain (unspecified but nevertheless flesh-and-blood) persons in a certain way. Whether it is true or false depends on what history actually is. The second does not attain reality because it is true no matter what history may be. It is a tautology, true for all possible worlds.

In spite of the absence of twentieth-century logic, readers of MIND will find this study of Aristotle's logic useful, because of its clarity and conscientiousness and scholarly method.

RICHARD ROBINSON.

Perennial Philosophers. By ARTHUR H. RYAN, D.D., D.Ph. Dublin: Clonmore & Reynolds, 1946. Pp. 71. Price 3s. 6d.

THE aim of Dr. Ryan's book, which originated in a series of talks broadcast from Radio Eireann, is to awaken interest in the lives and personalities of some of the greatest exponents of the theistic tradition in European philosophy. The distinguishing mark of this perennial tradition is, in his opinion, a belief in 'the transcendence of God, the immortality of the soul, the validity of intelligence and the freedom of the will' (p. 67), and, if we allow for the fact, noted by Mansion in a recent review, that neither Plato, nor Aristotle, had any technical term for freedom of the will, this characterisation of the tradition is perhaps as just as the complexities of the subject will permit. The lives with which he deals are those of Augustine, Boethius, Abelard, Aquinas, Socrates, Plato and Aristotle; but, not content to be merely a *laudator temporis acti*, he concludes his book with a short account of the neo-scholastic movement started by Cardinal Mercier at Louvain some sixty years ago, and now one of the most active schools of philosophy in the world.

The outstanding merits of Dr. Ryan's work are the verve with which it is written, and the vivid historical imagination that he reveals in his characterisation of each of these different thinkers. Like his compatriot Bishop Berkeley, he never leaves us in any doubt as to which side he favours, wherever he may take us, and whatever the causes in debate may be. He has also a wealth of information about historical events and places, of which he makes very effective use. The picture that he draws of the episcopate of Augustine is unforgettable, and will no doubt win many new readers for that incomparable master of the Latin language. No one, not even Edmund Gibbon, has expressed more vividly the unique position occupied by Boethius and the city of Ravenna in bridging the gulf between the Roman Empire and the Middle Ages. Though he exposes in the severest terms the sins of Abelard, Father Ryan shows a charity toward the sinner and his more creditable works that marks him out as an historian of thought whose views are worthy of the greatest respect. He tells the story of St. Thomas with many geographical and historical details that will be new to even the best instructed readers, and he shows a true understanding of his office as a teacher when he warns us that the very perfection of St. Thomas's philosophical technique makes his writings too unsensational for the ordinary reader.

The chapter on Greek origins follows that on St. Thomas, presumably because Father Ryan felt that this was the correct way to present the lives of Socrates, Plato and Aristotle to a predominantly Catholic audience with no prior philosophical knowledge. His admiration for Aristotle

is unbounded, though not uncritical, for he deplores the pessimism of his views on women and slavery. My own opinion, for what it is worth, is that the admitted excellence of Aristotle's discourse is a snare, and that the intrusion of Aristotelianism into the Middle Ages was a disaster, because it discredited the Empiricist reaction against dialectic that we can trace quite clearly in the Thirteenth Century Franciscans. To say this is not to disparage the work of St. Thomas, which was apologetic rather than scientific in its aim—an attempt to reconcile the teaching of the Church with the best thought of antiquity rather than an attempt to discover new scientific truths. After all, Dominicanism was itself an experiment launched in order to combat the ignorance and heresy of the Twelfth Century, and we have St. Thomas's words to vouch for the fact that the great experiment of Christian society was of far greater importance than the ideological system that he perfected for its use.

There are few people who will not be a little wiser for the reading of Dr. Ryan's book, and there are many who will be inspired by it to study for themselves the lives and writings of the great constructive philosophers with whom he deals. Let me, then, conclude by citing in paraphrase the three rules of argument that he mentions at the end of his book.

(1) Never criticise any point of view without having gone to the trouble of fully mastering it.
(2) Never allow yourself to say or write for those who agree with you what you would not be ready to say or write for anybody anywhere.
(3) Never indulge in the kind of remark of which the only pungency is the power to wound.

Discussions conducted according to such rules would soon convince people that there is a difference between wisdom and folly, and that the judgments of a wise man like Father Ryan are worthy of more respect than the slogans of publicity agents, however often, or however loudly, they may be repeated.

A. T. SHILLINGLAW.

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